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Scientific Method

An Inquiry into the Character and Validity
of Natural Laws ;

By

A. D. RITCHIE

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"Le me pare che quelle scienze sieno vane a ~~piane~~ di
errori, le quali non sono nati dell' experienza, madre di "
ogni certezza, e chi non terminano in nota experienza " "
LEONARDO DA VINCI

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PREFACE

PHILOSOPHERS who write about Science and scientists who write about Philosophy are too often preoccupied with the scientific theories and discoveries of the moment to the detriment of both their Science and their Philosophy. No single mind can contain the whole of the results of modern scientific investigation, so that any man's opinion on most topics is bound to be superficial and based on hearsay. Outside his special province the opinions of the specialist are no better than those of anybody else. Therefore, let it not be thought that this essay attempts to survey the present state of scientific theories or to test their validity, or that it aims at proving that some system of Philosophy is true because some theory of Physics or Biology is now fashionable. Such attempts I gladly leave to others.

The task I have undertaken, though needing no special technical qualifications, is really much more ambitious. Namely, to state what kind of reasons there can be for holding any scientific theories whatever, whether they are those of Pythagoras, of Newton or of Einstein. The attempt must fail, but the failure will be honourable, if it brings home to any reader the interest and importance of the problems considered, and breeds in him a distrust of cheap and easy solutions.

I should hardly have been aware of the existence of the problems dealt with but for the work of four recent thinkers—Moore, Russell, Whitehead, Broad—whose influence is apparent on almost every page of this book. If there are pages where the influence is not apparent, the judicious reader will skip them. Besides these authors I must mention particularly Mr J. M. Keynes and Dr N. R. Campbell. Chapter III represents what I had to say about Induction before reading Keynes' *Treatise on Probability*, Chapter IV is the result of reading it. It may be thought rash

to have dealt with Induction before the appearance of the final volumes of Mr W E Johnson's *Logic*, but the risk had to be taken. Such ideas as I have acquired on the subject of Measurement are the outcome of reading Campbell's *Physics* *The Elements*. While everybody pays lip service to the vast importance of measurement in scientific investigation, Campbell alone seems to have thought seriously about the process. I disagree with most of his views, but had I agreed more I might have thought less. I must confess, however, that my treatment of measurement seems to me the weakest part of this book.

Perhaps it is necessary to defend my attitude in refraining (as a rule) from destructive criticism of views I disagree with. Destructive criticism is an amusing and healthy exercise, it excites both hunter and hunted and hurts nobody, but it is somewhat futile, except as an irritant and disturber of dogmatic slumbers. Apart from this effect, a thinker's most valuable critics are those who agree with him. They at least have some chance of knowing what he is talking about; hostile critics have hardly any.

The quotation with which the last chapter finishes is taken from a paper by Dean Inge (*Proc. Arist. Soc.*, 1918-19, p. 272) and is out of the writings of Pennington. The quotation on the title page is also taken out of an article and I have failed to verify it from those works of Leonardo's available to me. But I have found many similar expressions in his *Notebooks*, though no single sentence that hits off the idea so well. Leonardo da Vinci, were he consulted, might repudiate the actual words, but I am sure he would not repudiate the thought.

The present essay has its origin in a dissertation submitted for the examination for Fellowships at Trinity College, Cambridge, in 1920. Since then the greater part has been rewritten.

A D RITCHIE

Manchester,
February 1923

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CHAPTER II

The External World

§ 6. OUR knowledge of the external world is derived ultimately from what is presented to us in experience ; but the processes by which the raw material of sense is converted into the manufactured products of scientific theory are complicated and difficult to follow out. Much that is originally given in experience must be left out, and much that is apparently not given must be put in ; in fact it is difficult to find any signs of the raw material when we examine the finished product. This difficulty is part at least of the cause for the Bifurcation of Nature against which Whitehead so very justly protests. There has for long been a tendency to relegate the immediate termini of sense awareness to one world and the entities of scientific theory to another, leaving us, not with the system of Nature which it should be the business of science to explore, but with a dream world of sense on the one hand and a fairyland of conjectural entities on the other and nothing apparently to bridge the gap. I have nothing to add to what Whitehead says in the first two chapters of the "Concept of Nature," and shall endeavour to adopt the point of view advocated there. We must start then with a world of sense presented to our observation through our five senses. It is the business of science to explore this world as it appears and to point out what it is really like ; and it is not the business of physical science to discuss how we come to be acquainted with this world or what processes go on in our minds. Physical science is concerned with nature as apprehended not with our apprehension of it. It is of course

concerned with the processes by which our bodies are affected by other bodies, because these are all parts of nature.

If in the course of enquiry into the relations that hold in the world of sense experience, some of its native hues become "sicklied o'er with the pale cast of thought" that is unfortunate perhaps, but, in so far as the relations are not obvious but concealed, the pale cast of thought is inevitable and is the price paid for knowledge. We may find that we have to invent more than is *prima facie* actually present in experience, but if our inventions give valid information then they are real inventions and take their own place as genuine ingredients of nature. There is I think no need to discuss the matter further at present. One question, however, must be discussed and that is why certain ingredients of sense experience come to be left out of account. Is this a matter of necessity or merely dictated by convenience, by the need to tackle easy problems first and hard ones later?

There are many classes of events that lie wholly outside the scope of ordinary scientific investigation, such as dreams and illusions. Yet they are just as much a part of our experience as any of those parts that are studied. Not only are dreams left out of account but they tend to be relegated to an unexplored world of their own outside the limits of nature. When Heraclitus¹ said, "The waking have one world in common, but the sleeping turn aside each into a world of his own", he put his finger upon one of the awkward points about dreams. Scientific procedure can deal only with what is common to all men. Poincaré² actually defines science as being what deals with the objective and explains that what he means by objective is having relations common to all men.

Every experience is as such perfectly private to the percipient. No one can share his sensations with any-

¹ Fragment 95 Burnet, *Early Greek Philosophy*, p 153

² *La Valeur de la Science*, Pt III, § 6

body else. Where then is the common or objective element? If it is some point or aspect of what is experienced that is common, why do we deny objectivity to some experiences and allow it to others? It is no answer to say that the act of experiencing, the mental part, is private and the object or thing common, because that does not solve the difficulty of delusions and dreams. Besides it amounts only to saying that objects are objective, a proposition that sounds like a tautology, but is not even true.

When I look at a patch of grass, the actual quality of the greenness I see is perfectly private and particular and incommunicable, and so also is anything experienced through any of the other senses. Not only can I not communicate the actual unique quality of my experience to other men but I cannot transfer it in any way from one of my own experiences to another—not directly, that is, only indirectly through memory. It simply comes and goes and there is an end of it. But there are relations within the whole composing a single experience and holding between different experiences, and it is these that are common, communicable and public. Among the most elementary of these are order in time. It is a fact that we can talk intelligibly about our experience and understand one another. This does not imply that all relations are the same for all individuals. Temporal and spatial relations differ to some extent as between one observer and another from the very nature of the case. Nobody can see from quite the same point of view as another. Moreover individuals differ in their power of discrimination of objects. The first difficulty it is part of any rational theory of space and time to get over, but it is commonly got over for practical purposes owing to the fact that in a vast number of cases different observers can attain a point of view so nearly identical that the differences do not matter. The difficulties that occasionally arise are explained away with greater or less plausibility. For

instance if another person and myself look alternately through a pair of field glasses, differences in time and position do not prevent our being able to assume that we are looking at substantially the same things, although exact analysis of what happens will show that we are considering only similar things not identical things. When we see things that are obviously different we generally manage to agree that some change is taking place externally to ourselves about which we do not differ. If for instance my friend looks and sees a boat to the right of a certain buoy, and then I look and see it behind and he looks again and sees it to the left, we do not quarrel about this, we agree that the boat is moving.

Differences in power of discrimination of qualities are rather harder to deal with. For instance if my friend looks through the glasses and says the buoy has got black and white vertical stripes and I can only make out a greyish object bobbing up and down against a background of sea, what happens then? Of course we may just quarrel about it, he maintaining it is striped, I that it is plain, but if we have any sense we shall try and agree about it. We may be able to argue which it is likely to be from our general knowledge of colours of buoys or our previous knowledge of the place, but even if any such external source of information is lacking the point is still capable of settlement. We shall almost certainly agree that the one who detects differences is right and the one who perceives only similarity mistaken. If my companion can discriminate differences in the field of vision that I cannot, the differences are there and he has better sight than I have. The matter can of course be decided finally by altering the conditions so that the differences become discernible by both observers. In the example just taken we can go nearer the buoy and then we shall both see the stripes. Anybody who has talked about colours to a colour blind person will have found that there is no difficulty in coming to real agreement (apart perhaps from a little confusion over

names) provided both are aware that there is a difference in the power of discrimination of the colour blind and the normal person. Disputes can only arise if it is assumed that the power of discriminating is the same. There must of course be some common ground of agreement to begin with, but fortunately that is usually quite easy to find.

If somebody says he can detect differences we are prepared to believe him. If he says there is no difference where we can perceive it, we distrust him at once. In fact that is a crucial test of any theory. A theory which says that things that appear to be different are the same is false, but one which says that things which appear to be the same are different may very well be true. Diversity is directly experienced and is indubitable; identity is not perceived directly but only vague similarity from which identity may be inferred. The great and almost instinctive confidence placed in microscopes and telescopes and any instruments that increase our power of discrimination is probably due to that cause.

A fraudulent observer who maintained he could perceive differences in objects not perceptible to others could never be detected as long as he confined himself to this assertion and drew no inferences from his alleged experiences to what was admittedly capable of discernment by ordinary men. But then his apparent superiority would not make any difference. If he did make inferences as to things discernible by others, as he almost certainly would, it would be possible to test the validity of his claims by experiment. For instance if a man claimed to have a sixth sense he would only be sure of escaping detection if his sixth sense objects possessed no other properties discernible by the ordinary senses. If he said they had it would be easy to set traps for him.

There does not seem to be any a priori reason why difference in the power of perceptual discrimination should always be capable of settlement; but it certainly

appears that as a matter of fact they are. A point of practical importance in this connection is the difference between a trained observer and an untrained one. The expert is nearly always able to discriminate better than the neophyte, because he knows what to expect. Of course, sometimes he may expect too much. It is a commonplace that even the expert, if he suffers from an overdose of theory, may see things that are not there. Nevertheless it is generally accepted that the expert observer is more likely to be right than the neophyte.

When we say that there are relations among observed things which are common to all men and about which agreement is obtained we must make one reservation. We must say that the common or objective relations are those perceptible by all observers who are properly situated. We say that the white corpuscles of the blood are objective, but we do not mean that anybody can see them just by looking at some blood. What we mean is that anybody who looks at the right sort of blood preparation through the right sort of lenses in the right way will see them. Actually things that are objective in Poincaré's sense may be extremely hard to observe and have only been observed once or twice. The planet Neptune is objective but very few have ever seen it. The cry of the Dodo is objective but nobody can hear it now however much they would like to ; because they cannot attain the proper situation.

There are many difficulties introduced by the notion of the proper situation of observers. Supposing a dipsomaniac says he sees pink rats, we have to admit that he has some sort of experience but we usually consider that the rats are not objective—that there are no rats in fact. We also say that where he is wrong is in the inferences he draws from his experience whatever it may be. When a sober man sees rats we find that his inferences about them are (more or less) correct while the inferences made about the pink kind by those who

see them are usually wrong. But the dipsomaniac might argue in this way: "You say my rats are not objective and that I make false inferences from what I observe, but that is not the real reason why you don't believe in their reality, because you often make mistakes about what you call real rats. Where you and I really differ is not in the matter of inferences but in the matter of situation. I can see pink rats because I have drunk the necessary quantity of whiskey. You have not drunk enough and so can't see them. It is just the same as with the things you see through microscopes. You can't see a white blood corpuscle without looking through a microscope, and you can't see a pink rat without drinking whiskey. What is more it is no use your pretending that you believe what you see through a microscope is real because of the optical theory of lenses, because it is ten to one you know nothing about the lenses or the theory".

This argument is not easy to refute, in fact I think the reasoning is perfectly sound. In the majority of cases our test of the reality of things is the sense of touch, but in the case of microscopic objects and the heavenly bodies touch fails us. They are purely sight objects, like the pink rats. If we can get the dipsomaniac to agree that he knows nothing at all about his pink rats except that they are pink and that he sees them moving about, and that any further inference he makes about them is probably false, then I think we can admit his argument. The pink rats, like the objects we perceive in dreams, will be constituents of the external world, but they will differ from the commonly accepted constituents in that we know practically nothing about them. Common and general relations do very likely exist but they are few and very difficult to find, and in fact have hardly been found at all. The only law we know of in connection with pink rats is that mentioned, that a large amount of drink must be taken in order to see them. In the same way we know of no laws about dream

objects. They do not obey the laws of physics, probably not even the laws expounded by Freud and his followers. But that does not mean there are no laws and that they have no relations common to all observers, merely that these laws and relations are extremely hard to find.

Anything that can by any possibility be an object of experience I would admit as part of the external world. But it is perfectly obvious that for the purpose of acquiring knowledge of that external world we are right to neglect those ingredients whose common relations are very hard to discover for those whose relations are easy to discover and are many and important. This is just what has happened. Certain parts of experience that obviously lend themselves to the purposes of knowledge because of their comparatively numerous and simple relations are examined in preference to others. It is only common-sense to tackle the easy problems first, but it is stupid to say the difficult ones do not exist simply because they have been left on one side.

Men of science are so preoccupied with the study of those parts of the external world which they actually do study that they are inclined to say there are no others. It is a fact that there are others, though the study of them has been left mostly to charlatans and fanatics. The delusions that these people have introduced make a rational study even more difficult than it should be and less inviting. The purpose of this discussion is merely to point out that such extra-scientific parts of experience do exist and might be of interest. Until they have been properly investigated there can be no scientific grounds for having any theories about them, but still less is there any scientific ground for saying they do not exist.

§ 7. The first task for the investigator of nature is to make some sort of a classification of what exists. It is most unfortunate that practically all the original work on this subject was carried out by our remote and simian ancestors, possibly by even more distant

philosophers who first left their homes in the Mesozoic Seas to walk on dry land and breathe the air. None of these thinkers have left any record of their investigations. Each one of us in early youth has gone through an abbreviated course of instruction on the same topics ; but we were not of an age to reflect intelligently on the process or to make any record of what was happening. It is therefore extremely difficult to find out anything about the first foundations of scientific knowledge or to make any statement about them that would be likely to carry conviction. Psychologists and philosophers have raised much learned dust in controversy on the subject, but have not got very far. However, a certain amount may be said with a fair degree of probability. The primitive and only half-conscious concepts that science takes over from common-sense are gradually, as knowledge increases, submitted to a process of enlargement and refinement. At first they will be exceedingly vague and of very limited application, gradually they will be used for new purposes and become modified and enriched and also more definite in the process. Consider for instance the concepts of Space and Time. These to the mind of a modern physicist would seem to be utterly different from what, corresponding to these names, is before the mind of a savage. But it is justifiable to give the same names to the ideas because we believe the physicist's idea to be the natural outgrowth of the savage's. It has grown in two ways, it has become both richer in content and more definite and more general in form. The physicist not only means more by Space and Time than the savage, but he comes nearer to knowing precisely what he means. The fact that the savage would not recognize and would repudiate the physicist's views is not to the point, because he has not had the training necessary to know what the physicist is talking about. What is of importance is that the physicist himself only uses his most refined and developed notions when the nature of the problem

in hand requires it. If he is making a simple physical measurement he does not utilize what he knows about "Geodesics" and "Point-Events", but is content with crude Newtonian ideas. If he is eating his dinner he is content with still cruder Pre-Newtonian ideas which are much the same as those of the simple savage. There is no contradiction involved; it is merely a question of saving of mental labour by using whatever notion is adequate to the matter in hand.

The proposition "Dinner is ready" is one involving certain spatial and temporal relations. But the notions of Space and Time possessed by a child of three are just as good for the purpose of enabling him to go and eat his dinner as are those possessed by the physicist. If the child says to the physicist "It is dinner time and dinner is on the table", we can say that both are aware of the meaning of what is said and it has the same meaning for both, for they act in the same way. On the other hand if the physicist tried to tell the child what was meant by the acceleration of a particle the child would not understand, because his ideas of space and time are not sufficiently developed, that is to say that degree of precision of analysis has not yet been reached by him and he would very likely think (in secret) that the physicist was talking nonsense. It is difficult at first for the individual who has thought and acted at the poorer and vaguer level of analysis to grasp what is required to reach the more precise and higher level, and he is apt to think it is really something different that is meant. But having reached the level of greater precision he then sees that everything that was meant before is meant still and more too, though possibly a few modifications are required in certain directions. But it must be clearly understood that the vagueness or precision is purely relative to the problem being investigated. Ideas that at the present day seem to be the very summit of exactness will be found to be relatively vague and indefinite and inadequate and even

erroneous when further problems have to be dealt with. The fact that we do not know clearly what we are talking about does not prevent our talking sense, or knowledge would be impossible, but it is likely to lead us into error some time or other, and then a certain clarification of ideas is required in order to get over the difficulty. It is absurd to despise our predecessors because their notions were crude. If they came to correct conclusions as the result of them it does not matter how crude they were.

It may be concluded that a detailed analysis of primitive ideas about the external world is really entirely unnecessary either to understand the most developed ideas or to ascertain their validity and adequacy. As long as a notion leads to satisfactory results it may be as vague as you like, it is only when errors arise that it must be expressed more precisely or else dropped altogether, to be replaced by some other. An analysis of primitive ideas in terms of modern scientific language would be quite impossible, simply because they are perfectly vague. The attempt to define them would simply consist in stating some modification of the present accepted definition of the scientific idea that was thought most closely to resemble the primitive one. Therefore no attempt need be made to show what is the primitive classification of the external world; it is sufficient to describe what must be the method of attacking the problem, for that will be the same at any stage. For instance, I am not concerned in the least to try and expound how men have come to have notions of space and time, that is a matter for the psychologist. Nor am I concerned to expound what is really meant by space and time in modern physics. This is a question for the physicist and the philosophical aspect has been admirably dealt with by Whitehead (*Principles of Natural Knowledge*).

The first point to be noticed is that there is no essential difference between what we call a general

concept and a law of nature, between say the notion of Time and the Law of Gravitation.¹ When a law of nature becomes very completely embedded in the body of scientific thought a single word is used to cover it. At the present day the word "weight" might be said to stand by itself for the Law of Gravitation. Of course it is not quite correct and logical simply to say that a general conception or an idea is the same as a law of nature, because a law of nature is properly asserted as a proposition and when we speak of a concept or idea it implies that we do not trouble to assert any proposition. Without attempting a full discussion of a very difficult subject, I would put forward the following scheme. We have on the one hand certain "facts" which we may conveniently, though not quite accurately, treat as being simply given in sense experience. On the other hand there are certain beliefs "about" classes of facts which are right or wrong according as they do or do not "correspond" to the facts. Now the full expression of a belief is provided in an asserted proposition, the proposition itself is of course neither a belief nor a fact, or if it is a fact it is one of a very different kind to the facts of "nature" we are considering. It is, however, only in certain cases and for certain purposes that we take the trouble to assert a proposition expressing our belief. It is usually a sign that we are in doubt. Thus it comes about that probably no proposition ever has been asserted to express some of our beliefs, among them our most general and deep-seated beliefs. And it is in many cases extremely difficult to find a proposition which will express the belief at all adequately. It is much easier simply to let a name standing for the conception or idea we have in our minds take the place of the proposition. For

¹ This was I think first clearly stated by Whewell in his *History of the Inductive Sciences* and further developed in his *Novum Organum Renovatum*. But the point is rather obscured by Mill, and as regards Space and Time at least, completely obscured by the Krypto-Kantian view put forward by Karl Pearson in the *Grammar of Science*.

the most part no difficulty or confusion results as long as it is realised that the name of the concept is merely a symbol, symbolizing a proposition which remains implicit. We may say in a vague sort of way that everything in the physical world is in space and time or that all objects of perception have spatial and temporal relations, and it is assumed that everybody has an idea of space and time in virtue of which they can understand what is said. But to state a proposition which will really express what it is we believe, in virtue of which we apply our idea of space and time to everything in the world, is a very difficult matter. However, it will probably be admitted as correct, that the proposition would be of the form "that there are things of a certain kind and that all of these have relations of a certain kind", which is exactly the form of any statement of a law of nature. The same could be said about the primitive ideas about "bodies" and "substances" and so on. In all that follows, therefore, I shall treat general concepts or ideas "about" the external world as identical for all ordinary purposes with what we mean by a law of nature. A law of nature, I take it, is a general proposition which asserts (more or less adequately) that certain relations always hold in certain cases. And this proposition is believed to correspond more or less to actual fact, and it is in virtue of its correspondence that it is said to be true and a law of nature.

The laws of nature, since they are assertions of general relations among "kinds", depend upon the classification of the objects of the external world. The process of classification must be considered as preceding the process of formulating natural laws, though historically the two processes develop together, and logically they are intimately connected. Any event in the external world as it appears to any observer has two aspects or parts, one private and unique, the other common and general. However important the unique

and private aspect may be for other purposes it is of no use for knowledge which shall be general and communicable, as is scientific knowledge. It is therefore necessary by analysis to extract the common and general aspect and consider it in isolation. Now the common aspect as we have seen is given by certain of the relations which are common to all observers or in which a common element can be found. These relations constitute the general character of an event.

In considering the general character to the exclusion of the unique, we do not necessarily neglect the particular character of an event. By the particular character I mean its position in space and time, its place and date. These are unique only in a very special and restricted sense, in that the whole of space-time is a complex of events such that no two parts are exactly alike and each event can be identified by means of its place and date relative to others. Given a starting point we can describe any event without ambiguity by its position. So far place and date are unique, but it is a purely external uniqueness of relations that serves for identification. The spatio-temporal relations of particulars are themselves general. If A is to the left of B and B to the left of C, the relation "to the left of" is general and is identical in both cases. Therefore, though all events are particular in that they occur at one special time and place, all their spatio-temporal relations are general and are such as form proper objects of scientific study.

Not only can we ask the questions "where?" and "when?" about events but we can ask "of what sort?" Events not only have a place and date they have also a quality. The individual quality of an event is certainly private to the observer. But events are grouped in classes according to their qualities and some of these classes are common to all observers and are general. No two people may have exactly the same quality of

experience when they see something green, but they will agree perfectly as to what things are green and what are blue. The common and general aspect of the quality of events is their mutual relations of similarity and dissimilarity which make them members of classes or aggregates.

The qualitative character of events stands on a different footing to the spatio-temporal character and is not a part of space and time. But that is no reason for dismissing this aspect of things from the external world and attributing the qualities of things to the mind of the percipient. It is true that the progress of science tends to give greater and greater emphasis to the spatio-temporal aspect of things and less to the qualitative and even to reduce the qualities of events to spatio-temporal relations; but that does not imply that the qualities are no part of the external world, it merely implies that they are less tractable parts than the others. We start off by perceiving things as coloured and hot and hard, and then for scientific purposes drop the consideration of these characters for related properties of molecules and electrons which shall be as far as we can make them purely geometrical. That does not mean that the things are not really coloured and so on, but only that for purposes of knowledge these are not themselves very useful characters.

Supposing we could consider the world of immediate experience just as it appears to our five senses and answer the questions When? Where? and What sort? about everything in it, then knowledge would be complete without the need of any theoretical constructions. But obviously this course is not possible. All the human beings that are or have been or ever will be only experience an infinitesimal fraction of what there is to experience, and the small portion they do experience is confused and varied beyond all description. Therefore we must have recourse to theory to shorten the task and make it possible. The first bit of theory is the

invention of what Whitehead calls perceptual objects—a theory we must examine.

First of all there is a possible objection to be dealt with. If anything that any percipient can possibly be aware of is a real ingredient of the world then either what is experienced must be created by being experienced, which seems a hard doctrine, or it may be objected the universe would be uncomfortably crowded with actual and possible data of sense. Philosophers have certainly had a mania for emptying the Universe. They have always wanted to show that this, that and the other do not really exist, but only certain pet entities of their own. Without putting it forward as more than a personal prejudice, I should say that one of the few really delightful things about the world is that it contains so much.

"The world is so full of a number of things
I'm sure we should all be as happy as kings"

Even if we have to find room, not only for all that our senses may perceive, but also for all the theoretical constructions of science, that need cause no alarm. Provided scientific theory is right then the molecules and electrons must also be genuine ingredients of the world although they belong to a different logical type. It is no explanation of their status to say they are logical fictions. A fiction is something false and cannot be logical. If they are really logical they are not fictions. They cannot even be convenient fictions, because fictions are never convenient in the long run. Even in science honesty is the best policy. The belief that scientific entities do not exist is partly due to not realizing that the entities of common sense such as tables and chairs are theoretical constructions themselves and not immediate data of sense. If they were primitive entities then perhaps there would be no room for the scientific constructions. But for the two sorts of theoretical constructions from events there is plenty of room.

§ 8. Probably the most elementary distinction we

can introduce into the external world, which is prior even to our doctrines of time and space, and is the basis of all subsequent classification, is the distinction between Universals and Particulars; a distinction that can be utilized without pronouncing judgment for or against any of the rival theories of Nominalism, Conceptualism and Realism. It is remarkable that for most men of a speculative turn of mind the particulars seem worthless compared to the universals. The particularity of the particulars is their bondage to space and time; they are at one particular place and time and no other. The universality of universals is their freedom from space and time, and it is this that constitutes their charm and their weakness. Their charm, in that knowledge of universals enables us to make statements about all or any of a kind: their weakness, in that we never come across a kind but only particular instances of kinds. Now it will not be disputed, I think, that any method of analysing the whole of what we experience is legitimate and gives us valid knowledge. We cannot falsify except by asserting that things are not experienced, which are, or that things are experienced which are not. As long as we do stick to experience we are bound to be all right. It will also be admitted that as experience is infinitely varied there are an infinite number of ways in which it can be analysed. It is therefore remarkable how little variation there is in the way mankind make their analysis. It may be all a wicked conspiracy to deceive the philosopher, but it is more likely that the way people as a matter of fact treat nature is the easiest way and the most successful yet discovered.

The presented whole of experience can be discriminated in a number of different ways, giving rise, according to the method used, to different types of entities. The main types of entity are *Events* on the one hand and various types of *Objects* on the other. Events in Whitehead's sense are practically what are normally called particular *facts*, but there is some con-

fusion in the ordinary uses of the word *fact*. Events have the property of *extension* and they also *pass* continuously into one another. This passage or flux of events is the most striking point about them. There is no element of possibility or supposition about an event, it just is and no more. Events are not usually named except in virtue of their relations to certain objects, and no attention is paid to them as such but only to them in so far as they are taken as constituting the relations of objects. Space and time are the only developments of events by themselves and their relations that men have thought it worth while to trouble about. The reason for this comparative neglect is obvious, events are transitory, the past is irrevocable (as Whitehead says, an event is what it is, when it is and where it is), and more important still the relations of events are complex and particular, Space and Time excepted. Attention is therefore turned to objects, but as Whitehead points out, in ordinary and even "scientific" use no clear distinction is ever made between events and objects and hence the common fallacy, among others, that objects are "in" space and time, whereas objects are really only related indirectly to space and time in virtue of their relations to certain events. An object is strictly permanent. What is called the change of an object is the variety of its relations to the events passing in space and time. A moment's reflection will show that it is only because an object is permanent that we can observe change in its relations, and because an event is not permanent but passing and its relations fixed, that it does not change. When we say that an object is permanent we do not mean that it lasts a very long time or even infinite time, but that it does not "occupy" time at all. And it does not of course "occupy" space either; only events "occupy" space and time.

Whitehead enumerates four main types of object (1) sense objects (2) perceptual objects (3) scientific objects

(4) percipient objects.¹ The last class corresponds to the "observer" familiar in discussions of Relativity and does not concern us at the moment. The first class are the simplest and most primitive objects one can have. They are "what" is directly perceived and are given names such as "red", "green", "hard", "soft", (I know these are adjectives, but they are names none the less). They have the character of permanence which the events did not have. That is to say we can *recognize* a red thing or a soft thing. Recognition in so far as it means recognition within the *specious present*—the perceived present that is, not the theoretical instantaneous present—may be treated as primitive, though when it involves an act of memory it is presumably derived and complex. Still we have in all recognition, however elaborated, a primitive element, the recognition of what in any present is not involved in the flux of events. That is not to say that the object does not change, *z.e.* that its relations are constant, but that "it" is permanent. The defect of simple sense-objects from the point of view of their utility for acquiring knowledge is that their relations are complex and particular, whereas simple and general relations are required. Consequently at some prehistoric epoch the greatest of all scientific discoveries was made, the discovery of *perceptual objects*, as Whitehead not very felicitously calls them. These are the "things" of daily life—houses and chairs and trees and animals and so on. These are recognized by means of certain specific sense-objects or relations of sense-objects, and they are soon discovered to have certain fairly simple and general relations among each other, the relations themselves being events or relations of events. But

¹ An object in the sense used by Whitehead is a universal, whereas the common use of the word does not distinguish clearly between the universal (the class of events or objects) and certain related particulars (complexes). A complex is an aggregate of "things" which have certain spatio-temporal relations and is itself a "thing" and has its spatio-temporal relations. A class on the other hand has no spatio-temporal relations.

more extended and refined investigation shows an incurable confusion and vagueness in objects of this sort. The relations between them and sensory objects are confused and difficult to elucidate, hence the long-standing controversies about *primary* and *secondary qualities*, *naïve realism* and so on: The relations between the perceptual objects themselves—causal relations—seem to elude exact analysis. There is the further difficulty, a very obvious one but possibly not really serious, of the temporal limitations and change of perceptual objects. Take Whitehead's example of a sock. At some period of time we have only what we describe as a quantity of wool. By the process of knitting the wool becomes a sock, but no very definite transition point can be assigned when the wool ceases to be merely wool and becomes a sock. The completed sock by process of wear loses much of its wool and has to be darned. But even after much darning, when very little of the original material of the sock remains, it is still called the same sock. But finally a time comes when the name can no longer be applied. The whole process of transition is however quite indefinite. To this particular defect of perceptual objects much thought has been devoted by philosophers.

The legitimate objection to the changeableness of objects is that it introduces into the relations of things considerable complications which it is desirable to eliminate. For the purpose of acquiring knowledge it is always necessary to find simple and general relations and to select such objects as display relations of this sort. Consequently it has been found necessary for many scientific purposes to replace the perceptual objects of common sense by *scientific objects* which are conceived as composing the perceptual objects. These scientific objects are Particles, Molecules, Atoms, Electrons, and what not. It is not a matter of great importance which is the most fashionable kind of object at the moment, all have their uses for the solutions of

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certain problems, and all probably have their defects when it comes to the consideration of other problems. The great advantage that is gained by the use of scientific objects is that their relations to one another and to events are simple and general. The fact that the relations to sense objects are remote and not very simple is not of great importance.

Let us consider for a moment the world of scientific objects and events. The events provide the aspect of continuity; they form a continuum out of which the ideas of space and time may be derived by perfectly legitimate reasoning, as Whitehead shows. The objects on the other hand provide the element of atomicity and of permanence. Together the two sorts of things supply those simple and general relations called Laws of Nature. Theoretically, if science were sufficiently developed it would never be necessary to appeal to perceptual objects. It should be sufficient to state the laws in terms of scientific objects and events and deduce particular relations of sense objects and events, *i.e.* particular spatial and temporal relations of sense objects, which are amenable to direct observation. It must be remembered, however, that laws of nature can be and often are stated in terms of perceptual objects and events, and the only objection to thus stating them is that they are not so definite or general. But up to the present a very large proportion of the scientific knowledge available is stated in such terms and is adequate for the solution of those special problems it is designed to solve, within certain limits of accuracy.

§ 9. It will be readily admitted as characteristic of scientific procedure that the problems investigated are always defined and limited in scope. Nobody tries seriously to find one solution of all possible problems but only to find special solutions of special problems. The first question to be dealt with is how it is possible to assume that this sub-division of nature for purposes of investigation is legitimate. Why, for instance, does

an investigator who is studying the properties of chlorine pay no attention to the phases of the moon or the flooding of the Nile? There are several possible answers to this question. We may say (1) that we have some *a priori* knowledge or intuition which enables us to make the subdivision; (2) that we make subdivisions at random and then find empirically that one or other of the ones tried is correct; (3) that we can apply some methodological rule to the process, and by methodological rule is meant a rule that is neither empirical nor of the character of *a priori* knowledge about things, but has merely subjective value as a guide to enquiry. There are fairly strong arguments both for and against all three of these answers. One might be tempted to answer cursorily that as human beings are only capable of attending to quite a few things at any one time, problems must be tackled piecemeal whether there is any justification in reality or not; and this would be tantamount to a justification on methodological grounds.

Perceptual reality as a whole is too vague and fluid, too various and rich to satisfy our needs at once. Our needs, that is to say our aims when we are seeking knowledge whether for practical purposes or for its own sake, are fairly definite. I have said already that we look for simple and general relations, but the matter can be expressed more concretely than this. We are not content just to sit and experience whatever comes to us. We want our experience at any time to signify for us things not experienced, because spatially or temporally remote, but intrinsically capable of being experienced. We want to know of those relations, if any, which enable us to anticipate and successfully interfere in the flux of experience and that is what a knowledge of simple and general relations enables us to do. It is not enough to know that this particular element of experience is related in some way to that particular element, because we shall never meet with

these particulars as such again, we must know that this *sort* of particular is related to that *sort*. We must be able to analyse and classify and define so as to obtain general knowledge. The reason for desiring simplicity is obvious.

The vague perception with which we start is not properly speaking either particular or general; it is only by analysis and abstraction that particularity or generality can be found. The sense-objects of Whitehead are general, that is to say an object is a class of experiences. The particularity is provided by the events which are the situation of the object and are what clothe the object with flesh and blood. *Without its related events an object is a mere name.* The very first result of analysis is to separate the general from the particular and is in itself the beginning of a classification. For sense objects are found to be related to one another, and these relations by analysis and abstraction can be further generalized into a perpetual object; and so on. Once we have obtained universals by abstraction further consideration and abstraction will give us universals of a higher type, classes of classes for instance. From the classes red and green and blue we can attain the class of colours. Processes of this kind are purely analytic and are what are usually called classification. But it is also possible to proceed synthetically by means of external relations of objects to constant fresh types of universals. The step is made in this case by means of laws. We find as a matter of experience that certain colours and certain shapes are associated and hence construct a class of things whose characteristic it is to show this relation of shapes and colours. This is also a form of classification but it differs from the first example in that it assumes the existence of laws, *i.e.*, of general external relations among classes. In actual practice it is not always easy to disentangle the part played by assumed laws and that played by analysis pure and simple, though it is important to do so because

the laws are always fallible while the analytic procedure is not fallible as such. But the way that analysis is made depends upon what laws we expect to find or think we have found. That system of classification is chosen which will lead to successful inductions and any others will be rejected.

In the biological sciences the criterion of the naturalness or otherwise of a system of classification is simply the number of true general propositions of an inductive type that it enables us to assert. For instance if animals were classified simply according to the number of legs they had, that is as legless, two-legged, four-legged, six-legged, eight-legged and so on, although some general propositions about the properties of all legless animals and all two-legged animals would be found to be true, they would be practically only such as could be deduced from the terms of definition of the class, and few, if any, real inductions would be possible. But with a natural system the number possible is very large if not infinite. About the class Birds an infinite number of inductive generalizations are possible. But if we include men along with birds in the artificial class Bipeds hardly any can be made. We can say "All bipeds are warm blooded," but we find that that is possible simply because they all happen to be included in a larger natural class of warm blooded animals. A class that is not natural will do just what it was designed to do and nothing more.

It is not possible, I think, to say definitely how or why we can be justified in making an analysis and a classification of the real and of expecting genuine knowledge by this process. It must be accepted as a fortunate fact that the process does work somehow or other, and it is simply a problem of finding by trial and error what is the method that gives the best results.

If we are permitted to assume that there are perceptual objects which are natural kinds and general relations among them which we call natural laws, it is

fairly easy to see how we can proceed to subdivide the external world into special and finite problems for purposes of study. First of all we neglect anything that does not ostensibly constitute a natural kind and confine our attention to some particular kinds and study them and their relations. We study them first in order to classify them as requisite and then in order to find what natural laws, if any, there are uniting them. If our investigation shows what we thought were natural kinds are not, no irretrievable disaster occurs, we merely have to reconsider the primary classification we started with. If no natural laws are apparent within the particular region selected, again no disaster occurs, all that is required is to widen or alter the confines of the region until such laws become apparent. When the ancient men of science first came across the phenomena of the tides they were not able to find any natural law governing them until they included the movements of the moon into their consideration of the problem. Having done so they would find an approximate correlation between the times of ebb and flood and the position of the moon in the heavens and also a correlation between the extent of the rise and fall and the moon's phase. As long as they neglected to consider the moon the whole process would appear chaotic; probably it would not occur to them to take account of the moon for some little time as its connection with the tides is not obvious.

In the biological sciences the expressed aim in classification is to display actual blood relationship between animals and plants and not merely to catalogue them in a convenient way. Thus if the classification is correctly done it will display the laws of heredity and development when these come to be discovered. But when a new species of animal is discovered considerable experience and skill is usually needed to assign to it its appropriate position among known forms, so that convenience is to a great extent sacrificed to the desire to

make the classification a basis of laws. If we consider an artificial scheme of classification like that of Linnæus we can see the difference. Using this system anybody without any previous training can, by simply counting the sexual elements of a flower, name it and place it in his scheme. But once that is done absolutely nothing more follows. The plant is merely catalogued for reference. It is only because the Linnæan classification can be made to serve as an index to a natural scheme that it is useful.

Compared with classification in biology the classification required in a science such as chemistry is extremely simple, but there are several interesting points about it.¹ The properties of any "body", that is to say the system of events, sense-objects and their relations which "make up" the perceptual object, are of two kinds, those we can change arbitrarily one by one, such as temperature, shape or motion, and those we cannot change without changing others as well. The former are called accidental properties, the latter specific properties. Chemistry is essentially a study of the specific properties of bodies, physics of the accidental properties. The basis of a chemical classification depends therefore on discovering what properties are specific and examining them. All bodies having the same specific properties will be of the same kind, in this way we arrive at the conception of different "substances." A further distinction is necessary. A body to be a substance must be homogeneous, all of one kind, that is to say, however it is subdivided (only gross mechanical subdivision is intended) the parts must have the same specific properties as each other and the original body. The number of different kinds of properties possessed by substances is limited and generally they all possess some kinds in common, the differences are therefore quantitative. The process of classification depends therefore upon making measure-

¹ Cf. Ostwald, *Fundamental Principles of Chemistry*, chap. I

ments of the specific properties of all bodies. It so happens that given certain assumptions as to the distinction between elements and compounds and solutions, all substances fall fairly easily into a simple system of classification and except in a few cases each kind is very clearly marked off from all others, and the constancy of the properties is such that any variations are so small as to escape measurement, *i.e.*, the specific properties are really constant within the limits of experimental error. This fortunate state of affairs is only true as a matter of fact and in most cases. There are bodies to which it is very difficult, if not impossible, to assign constants, for instance the complex organic colloids. But these exceptions do not affect the validity of the general system of classification. The important point to notice is that the aim of the classification is as before to find natural kinds. The naturalness of the kind is determined by the constancy of the specific properties, as long as variable results are obtained we consider that we are dealing not with a kind or a pure substance, but a mixture. The crude test of homogeneity on mechanical subdivision is applicable in a great many cases and in others more refined manipulations that are in principle the same, such as fractional crystallization and distillation, can be utilized. But in any case our decision that any particular substance is a natural kind or pure substance is an empirical generalization and liable to be upset by further investigation.

It must be admitted that some modification of the meaning of natural kind is implied in this use of the name. We say that water and common salt (neglecting differences between isotopes) are substances or natural kinds, but a solution of the two is not considered a substance, at least not in the same sense. This is because the properties of common salt and water are all perfectly definite and constant when measured in the proper way, but the properties of the solution are continuously variable within definite, but fairly wide, limits. The name

"common salt" is perfectly definite and unambiguous and refers only to one class of things; a solution of salt is however ambiguous and the character of the solution is not completely defined until an extra term has been introduced; that is until the concentration has been defined. If an unknown substance is investigated which has that particular kind of variability of properties associated with solutions we do not consider it a natural kind or pure substance, but treat it as a mixture of two or more substances which we then endeavour to isolate. However, a mixture if it is homogeneous like a solution, is a natural kind in the wider sense that any specimens of the solution have an extremely large number of properties in common which no other bodies not members of the kind have.

In the case of chemical classification, as in the case of biological classification, it will be seen that simplicity of method of classification is sacrificed to the desire to obtain natural kinds. Even if a whole crop of special difficulties is raised in a classification of this sort, it is worth it if the end of obtaining natural kinds is attained. In fact it might be said that the aim of a classification is not to obtain a way of easily labelling and recognizing objects but of so selecting the classes of objects that natural laws shall be found to hold between them. But in the case of chemistry the matter is very much complicated because actually the theories, or natural laws, and the classification have developed simultaneously and have reacted on one another. The chemical classification of substances would be a hopeless task without a chemical theory to guide it. The satisfactory classification of substances was only made possible by Boyle's distinction between elements and compounds, Lavoisier's theories about oxygen and oxygen compounds, and finally Dalton's atomic theory. In order to classify a substance it is not sufficient even to show what its physical constants are, it is also necessary to determine its formula in terms of ordinary chemical theory, for this

displays its chemical relations and also to some extent determines its physical constants.

The general aim of a natural system of classification is not merely to make a catalogue of things under convenient heads for purposes of recognition, but so to choose the classes that general laws shall be found to relate them. If such general laws are found the system is considered to be successful, if not, it is considered unsuccessful, and another one is substituted for it.

The question asked at the beginning of this section as to the legitimacy of subdividing the external world for purposes of investigation, which is the same as the problem of *irrelevance*, cannot be answered until after the treatment of Induction and will be returned to later.

CHAPTER III

Natural Laws

§ 10. BEFORE saying anything else about Induction there is one point I wish to make perfectly clear, on which it is impossible to lay too much stress; that is that the process of scientific discovery, of finding Natural Laws, which is what Induction is, is an Art, in the ordinary sense of the words. It is possible to state rules for extracting the Square Root of a number or for finding trains in Bradshaw, given which anybody not entirely devoid of intelligence can perform the operation in question successfully. But it is quite impossible to lay down rules knowing which anybody can write poems like Shelley or make statues like Praxiteles. So also is it impossible to lay down rules which will enable anybody to make discoveries like Faraday or Pasteur. But strangely enough Bacon seems to have thought it possible, and many later thinkers who ought to have known better seem to have had some such idea at the back of their minds. It is interesting to the Philosopher to try and state the rules that govern the successful carrying out of an artistic process, but the rules are of no help to one who has not got the artistic faculty and are unnecessary to one who has it.¹

In previous chapters we have got the length of classifying the things we experience. Observation soon shows us that one thing may be a member of several classes and that things have relations to one another; spatial and temporal relations for instance. By things

¹ This point has been very well stated by Dr N. R. Campbell in his little book called *What is Science* (Chap. IV), a book which cannot be too highly praised and is as good and brief as his other book *Physics, the Elements* is long and bad.

in this connection are meant Perceptual Objects. This at once suggests that there may be general relations between all the members of a class as such. That is to say that all members of one class may be members of another class also, or all members of a class may have a certain relation to something else, a class or an individual. It is obvious that the assertion of such general relations if possible constitutes a very valuable addition to our knowledge. For example we observe that there is a kind of bird we call the Cuckoo, and sometimes we hear her singing and sometimes not. On several occasions we notice that Cuckoos sing in Spring and we do not hear them singing at other times of the year. We are inclined, therefore, to make the general assertion "Cuckoos sing in Spring". Having made this assertion we are in a position to make such other assertions grounded on it as "There is a cuckoo, Spring has come" or "It is Spring, we shall hear the cuckoo". These things we could not say if cuckoos sang in Autumn or if there were Springs when no cuckoo sang. But it is clear that on logical grounds we are not justified in saying anything more than—"Given that there are cuckoos and a season of the year called Spring, the proposition 'cuckoos never sing in Spring' is false." We can infer that a certain general proposition is false but not that any one is true.

Unfortunately nobody is directly interested in proving general propositions to be false, except occasionally when somebody else has said they are true. We are more interested in the truth of the proposition 'Cuckoos sing in Spring' which has not been proved than the falsity of the proposition 'Cuckoos never sing in Spring' which has been proved. But the negative aspect is really the most important. Any particular fact, that is to say any proposition describing an event, is incompatible with certain general propositions, which therefore must be false, and is compatible with certain others, which therefore may possibly be true; that is always supposing

the event is correctly reported. Now an event may be described in a number of different ways all equally correct but differing in the general terms used and consequently in the general propositions that are obviously logically related to the event. Clearly if our reports of facts are to be of any value in establishing general propositions they must be so selected and so expressed as to demonstrate the falsity of as many general propositions as possible and thereby restrict the number of possible true propositions. The step from the observation of an event to the statement of a law or general proposition about it is immediate and almost instinctive. In fact it usually needs a special effort to describe an event simply as a particular event without dragging laws into the business; the effort is one that only the most sceptical and sophisticated ever make. No sooner have I heard a cuckoo singing in spring than I want to say that Cuckoos sing in Spring. In fact in one respect the statement of a law is merely a convenient way of summarizing the facts of experience. It enables us to say briefly what kind of experience we have had without bothering about describing places and dates all at length. It is only because a general statement logically implies assertions about events that have not been observed which may be and frequently are false, that we are at all cautious about the statement of laws.

Suppose we examine what happens in cases where a law is stated and turns out to be wrong. It must be made clear that no irretrievable disaster follows. The fact that it turns out to be false does not necessarily affect the standing of any other law. Neither does its falsity in the cases in which it is false affect its validity in the other cases.¹ This is a very important fact. If some particular law gives a correct description of what

¹ Keynes points out in his *Treatise on Probability* the fact that a law turns out to be false, does not affect its logical standing previous to the observation of the incompatible event. It may still have been the most probable law given only the previously available evidence. This point is one of the most important in his whole treatment of the matter and will be returned to later.

has happened in instances a , b , c and d ; even though it is false in case e it still remains correct in the other cases and may be quite properly described as *the* law of those cases. What we have to do therefore is to frame a new law which will cover all the old cases as well as the new, which will contain what was true about the old one without what was false. Obviously any law that has adequately described any event is not altogether false. It is better to consider it as badly stated, rather than false.

Take the case of the cuckoos. Suppose for several years we have heard cuckoos in May and June and have asserted that Cuckoos sing in Spring, then one year we hear the sound "Cuckoo" in December. Obviously our old law was wrong if all we mean by the name Cuckoo is something that makes a noise "Cuckoo". But if we are truly scientific we shall not rest content with this but will investigate further. We shall find that the cuckoo is not merely "a wandering voice" but a bird of a certain appearance and habits. The sounds we heard in December we shall find to be attributable to small boys and not to this particular bird. When a law is found to be false we naturally re-examine the instances as far as we can in order to find what restatement of the case will provide a new law. Very often it will be seen that the old law did not really fit the old facts very well. For instance if Boyle's Law was only examined in the case of permanent gases at low pressures it would seem to hold fairly well. But with vapours or gases at high pressures it will be found not to hold. Then a more careful examination of the first case will show that even there it is not an exact description of the facts but only an approximate one. The law that cuckoos sing in Spring we can express as "If x is a cuckoo x sings in Spring" and each occasion some cuckoo is observed provides some value of the variable. If $f(x)$ is any law, then $f(a)$, $f(b)$, $f(c)$ are instances of it. If it turns out false in any case, say $f(k)$ then it is

necessary to find some other form, f_1 which will hold in the instance k as well as a , b and c . If this in turn is false in some other instance, then we must look for still another form, f_2 which will hold in that case too. We can never be certain that the form we have found will really hold in every case whatsoever, but we can consider the possibility of there being one to which we can approximate. If we have found an imperfect law that holds in some cases even though not in others, we suppose that there is some law not entirely unlike that one, which holds in absolutely all cases and which we can postulate as a limit to be approached as nearly as possible. The fact that a law is true in any instance shows that it is not altogether alien to the truth and there is that in it if we can only discover it that is true absolutely.

There are no rules of course for substituting better for worse forms of laws ; it is a matter that must be left to our native genius, supposing we have got any. Anyway the fact remains that if an instance is found which contradicts an alleged law we do not rest until it is replaced by a better one that is not contradicted. A set of events for which there is no law is of itself an unsolved problem ; and unsolved problems are very painful and irritating things, like the little parasites that make the oyster grow its pearls.

The degree of confidence that is to be placed in a law is often said to be measured by the number of favourable instances, but this is obviously false. Many laws which are asserted with the utmost confidence are based upon a very minute number of observations. For instance the value of the Gravitational Constant G which is a very important law is known with great certainty and yet the number of relevant observations are few and are likely to remain few for all time ; yet we have no doubt as to the correctness of the values or that it is a constant and not a variable. There are many generalizations which are supported by countless

instances of which we are not nearly so certain. Of course it might be said that in the case of a constant like G that the general mass of mechanical measurements that are constantly being made that involve the assumption of the value of G are indirect evidence for its correctness. But that is evidently not all. It is easy to think of constants of which the accepted value might be wrong or which might be variable without much upset of ordinary physical laws and which we nevertheless believe in confidently as the result of quite a few observations, as for instance the atomic weights of some of the rarer elements. In any case the reliability of the laws resulting from physical measurements depends only to a very slight extent on the number of observations, but it does depend on the way the measurements have been carried out. What this means is that by carrying out a physical measurement in the correct way the chances of error and of discrepant instances having escaped observation are made extremely small.

The cases usually quoted in which the number of observations is important are such that it is only by repeating the observation very many times that we can have any confidence that negative instances have escaped notice. Innumerable observations of black crows have been made without our having any very extreme confidence in the generalization that all crows are black, because it is only by repeated observation that we can eliminate the possibility of white crows having escaped notice.

These two cases, that of the blackness of crows and the value of G , illustrate one point very well, what our confidence in the value of physical measurements is based on. In the first place we believe that the properties of bodies that we observe in physical measurement are such as lie at root of things, that we have somehow succeeded in arriving at the general ground plan of nature by their means. We do not feel that

the blackness of crows is something at all fundamental in the character of crows, it does not go to the root of the matter. Zoologists would not be greatly shocked if somebody found a pink crow. They would simply call it *Corvus Rubescens* and go on with their work. In the second place the precision of measurement consists in just this, that we can see so easily, if there is a mistake anywhere; the vagueness of non-metrical and qualitative judgments leaves more loopholes for error.

One of the reasons why we state laws is that they are simple; they serve as an abbreviation of experience, as Mach pointed out. For the sake of simplicity of laws we are even prepared to sacrifice a certain amount of accuracy. A law that is very simple and easy to use for purposes of calculation will often be preferred to one that is more accurate if that accuracy involves complexity. Thus in almost all calculations the simple form of the Gas Laws $PV = RT$ is used, although it is known not to be exact, in preference to the more complex but exact Van der Waal's equation. This illustrates another characteristic of laws that their function in a system of knowledge is to summarize experience and we are always willing to sacrifice accuracy to convenience up to a certain point. In particular if you know that a law is inaccurate there is no harm in using it. The only danger in its use would be if it was thought to be exact. One might almost state it as a general rule of method that errors do not matter if you are aware of them.

§ 11. The difference between one suggested form of a law and another intended to cover the same facts is a matter of classification. When an alleged law is found to be false in any given case the problem for solution is a reclassification of the facts in such a way that the anomalous one can be successfully included. This is a question that I shall return to later. In the meantime there are some possible objections to the views expressed to be dealt with. First of all, the

position may be summarized. Any observation that is made and recorded, is recorded in general terms. Therefore a general law is capable of being suggested by any observation. The very first time a cuckoo is heard to sing in Spring, it may suggest the law "Cuckoos sing in Spring." If the next Spring there are no cuckoos or if the next Cuckoo is heard in December, then the law is false, but all that happens in consequence is that we must try again. The passage in thought from a statement of a particular event in general terms, *e.g.*, "There is a cuckoo, and it is Spring" to a general law "Cuckoos sing in Spring" is as a matter of fact almost immediate and instinctive. It is only the frequent failure of the laws that makes us cautious and sceptical about the business.

It may be argued, much on the lines of Mill's famous attack on the syllogism, that the introduction of laws is really superfluous as the essential part of the process is one of argument by analogy, from particulars to particulars. Given a relation of an event A and an event B in some particular instance, when an event like A (A') recurs an event like B (B') standing in the same relation is expected. The alleged general law is not needed because we cannot know it to be true until we know it is so in the case of A' and B' about which the argument is. To take the ancient example, we cannot say that Socrates has died because all men are mortal, for all we know by observation is that various men die, and till we have found that Socrates has died we are not justified in saying that all men, including him, are mortal. We cannot state the law until the need for it has passed and it is at best a rhetorical flourish superadded to the argument. The reply to this contention is, I think, that as long as we stick to analogy of particulars any proper analysis of the problem is impossible. Such arguments by analogy are as likely to be wrong as to be right, and when they are wrong we must either give up the pursuit of knowledge altogether or else examine the

problem systematically. The statement of the general law involved is a necessary consequence of a proper analysis, for the general law is merely the general logical form of the facts involved stripped of the particularity of their situation. Any attempt at analysis, which does not result in formulating a law but treats the particular analogy as such, ends by making it look ridiculous. If we say that A is related to B and as A' is like A, B' which is like B will have a similar relation, and if we are wrong, as we may be, it is necessary to analyse the likeness. The analysis will consist in finding some respects in which A and A' are different and some respects in which they are identical. Obviously it is only in virtue of the identities that the analogy can hold. In so far as they are different the argument is false, but in so far as they are identical it is tautologous. If we have an A and an A' that are really identical, one part or aspect of that identity will be the relation to B and the argument is reduced to "A is related to B, therefore A is related to B". We are left with a problem similar to the original one about general laws, that we cannot make the inferences until we know by experience that the conclusion is true and the inference is not required. But what is more important, the process of analysis sooner or later involves the use of universals and of general laws. If Brown has died and we say Jones is like Brown and will die too, our analysis of the likeness of the two individuals will be expressed in terms of universals, qualities and relations, and we cannot avoid stating the relations or universals in the form of laws. The similarity between Brown and Jones that is relevant to the case in point is a matter of general relations among the attributes of the class men and the class of events called death.

¹ As long as we stick to argument from particulars to particulars by means of analogy we must give up the attempt to obtain knowledge in the instances where it turns out to be false. When it happens to be true no

analysis is required, naturally, but then knowledge does not advance by means of true assertions but by means of false ones. If a proposition happens to be true, our wants are satisfied indeed, but intellectually we remain where we were. Positive increase in knowledge only comes when a proposition thought to be true turns out to be false, as Poincaré says à propos of false hypotheses. If the inferences of Science were always correct, science would be useful enough but would be devoid of theoretical interest.

These contentions must not be taken to imply that analogy has no value; on the contrary, analogy is the basis of all inductive argument. All that is meant is that its use to argue from one particular case to another belongs to the uncritical and unreflected stage of reasoning.

§ 12. A natural law as has been maintained is essentially a general correlation between classes; it is an orderly arrangement of classes. Given the same basis of experience, the same facts that is, according as we select our classes, they may appear orderly or disorderly. The number of ways in which the data of experience can be classified is unlimited, and clearly it is necessary to select from them those particular ways of classification which will allow laws to be apparent. It seems possible that any method of classification would produce laws of sorts, that is to say that whatever the classes some general relations will exist between them. But it is obvious that some systems of classification will give more numerous and more general and simpler laws than others; and these, if anybody hits on them, will be adopted. This idea I wish to illustrate in detail. First of all I will restate what I believe to be the mechanism of the process. 'The allegation of a law is the allegation of a complete correlation in the case of some particular class so that we can say every A has the property or relation B. If we find any A that is not B we can either say that it is not really an A or that

what all A's really have in common is not B, but something else rather like it. In any case wherever a law is found to fail we try to reinstate it or some other like it by making a change in our classification.

First let us take a trivial case. It is a popular belief that red-haired people have bad tempers; this, if it were true, would be a natural law. At any rate it is undoubtedly a fact that there are people who are red-haired and bad tempered. There is some correlation between the classes, which, even if the belief as it stands is false, forms the basis of it. But it is also the case that some people whose names begin with B are bad tempered, yet nobody has ever thought that it was a law that all people whose names begin with B are bad tempered as they have in the case of red-haired people. The reason for this is clear. It is generally considered that the classification of men by the initial letters of their names is purely artificial. That is to say men whose names begin with B have that common and peculiar to them, but absolutely nothing else. Nothing would be more astonishing than to find as a matter of fact that among a large group of men all whose names began with B were bad tempered. It is more disconcerting to find a law where you do not expect it than to find it lacking in other cases where it is expected. We should scoff at a man who seriously investigated whether men whose names began with B were bad tempered. However clear his results and refined his methods of investigation, we should probably still scoff. On the other hand if he proposed to investigate statistically whether red-haired people were bad tempered we should have to admit that it was a reasonable subject for investigation, and we should admit this even if we believed that it was merely a superstition that red-haired people are bad tempered. That is because bad-tempered people and red-haired people are, to some extent at least natural kinds, classes that is, among which laws may reasonably be expected.

They are the sort of classes that may possess each a large number of properties common and peculiar to themselves, such as form the basis of laws. We know that there are bad tempered people who are red-haired, therefore the matter is a fit subject for investigation.

Suppose somebody has studied the matter statistically and found among a large group selected at random that 75 per cent. of the red-haired people were bad tempered while in the whole population only 50 per cent. were bad tempered. A result of this sort would show that there was something in the method of classification used, but not enough to justify the assertion of a law. Clearly in trying to correlate red-haired people and bad tempered people we have not got to the bottom of the problem and it still remains a problem. To make any further progress in knowledge it is necessary to revise the classification used and start again. And until the correlation is complete it is necessary continually to try new methods. For instance we might enquire whether red-haired people with Roman noses were bad tempered ; or we might adopt a different line and say that what degree of correlation there was, was accidental and due to red-haired people having been persecuted about it while young.

I have chosen this trivial example because it lies outside the region of our usual presuppositions about nature and the classifications involved are not in any way stereotyped. For the most part our classifications and a number of laws are assumed bodily. Prior to any systematic investigation of nature something must be assumed provisionally, and what is assumed will naturally be modified from time to time as experience shows to be necessary. But given any body of knowledge it is only reasonable that the modifications needed be carried out so as to cause the least disturbance to laws and classes already recognized. The process of rearrangement has been done so completely over such a wide range of subjects that it is easy to lose sight of the

fact that any alteration was ever needed. Though as a matter of fact there is probably no branch of knowledge in which laws may not be found to break down and in which classification may not require some revision.

It is sometimes maintained that very well established natural laws are raised to the status of axioms and are therefore immune from criticism in the light of any experience whatever. This is not, I think, quite strictly correct, though it is a fair approximation to what is true for the most part. It would be more nearly correct to say that a law supported by a very large body of evidence is not likely to be upset in any way to accommodate some comparative newcomer among laws. Generally speaking, it is worth giving a sprat to catch a herring, but the converse process is uneconomical.

When Galileo began to investigate the laws governing falling bodies, he found ready made a large body of knowledge about the external world which was relevant to his enquiry. Among others were a number of alleged natural laws and a fairly definite and coherent system of classification, the legacy of what may be conveniently called "common-sense". Much of this is summed up in the phrase "heavy body". To begin with he took this for granted, provisionally at least, without any elaborate analysis of what he was assuming. But he concentrated his attention on testing an alleged law, which may be stated in the form that the rate of fall of a heavy body falling freely is a function of its weight, so that the heavier body will fall the faster. To do this, as is well known, he took a 1-lb. and a 10-lb. shot to the top of the Tower of Pisa (he was a young man then) and dropped them simultaneously over the edge. To the great disgust of the self-styled Aristotelians present on that occasion, they reached the ground simultaneously or so nearly so that the difference was negligible. The alleged law, therefore, was false.

The next thing he had to do was to find a better law to replace it. It is important to notice that both Galileo and his opponents were all quite confident that there was a law to be found if only they looked for it in the right way. His idea of the right way of looking for the law was to observe how bodies fell under different circumstances, but most of his contemporaries thought the right way was to read the works of Aristotle.

The use of his five senses alone would have not sufficed him without the use of the classification of experience provided by common-sense and the known or assumed relations among the classes. But given the information supplied by common-sense the number of possible laws of falling bodies is so large that the correct law could never be arrived at by eliminating the possible laws one by one. Fortunately, a very small number of properly devised experiments suffice to disprove an infinite number of laws. Whenever an event can be repeated at will (within specified limits of variation) it shows that any law is false which asserts a functional correlation between the way the event occurs and anything that has changed between the various occasions. For example Galileo could easily show that the rate of fall of bodies was independent of the time of day, the direction of the wind, the phase of the moon, and so on. In this way in any investigation of restricted scope a whole host of irrelevances can easily be brushed aside. But there will still remain a large or more probably an infinite number of different possibilities, though they will be of a more or less restricted sort.

After finding that a number of different bodies under the same conditions fall at the same rate in spite of their differing in a number of respects, Galileo considered himself justified in asserting as a law that any body falling freely under gravity fell at the same rate, making the necessary reservations as to air resistance. He did this after trying only a small number of different bodies

out of the infinite variety of all possible bodies. } Nevertheless everyone will admit that he was right, and any objection to his procedure on the ground that though the law was true of certain lumps of iron and brass and wood that he had tried, it would not hold in the case of elephants or pearls or statues of Julius Cæsar which he had not tried, would be dismissed as frivolous. Nobody thinks he ought to have dropped an elephant off the tower of Pisa as well as his various kinds of shot. We should say that the respects in which elephants and pearls and statues differ from the objects Galileo actually used are not relevant to the enquiry.

The problem, however, is undoubtedly a difficult one and not to be dismissed lightly. It goes to the very root of our theories of matter. It cannot be denied that the whole structure of Physics is bound up in the experimental facts of the behaviour of bodies in the gravitational field. If the alleged facts were wrong there would be very little left of science, and yet it must be admitted that the relevant experiments that have been recorded are curiously few and that the number of bodies and substances that have been studied are also curiously few. It is very unlikely that the matter has been seriously studied since the seventeenth century, and the experimental facts bearing on the case have almost passed into the region of mythology. Of course every student of elementary mechanics does experiments designed to show the behaviour of bodies under gravity, but these though rather more precise do not demonstrate anything more than Galileo did with his inclined planes and do not include a much wider range of material. Of course the everyday behaviour of various kinds of mechanism and the familiar experience of weighing supply a certain amount of indirect evidence. The most that this evidence amounts to is that bodies of iron and brass and wood and some other common substances behave alike, and further that when frictional effects are eliminated the shape of a body is

not of any importance. What about comparatively rare or newly discovered substances? What do we know of the behaviour of Gallium or β -iminazolylethylamine in the gravitational field? These substances have been weighed to be sure, but that is not enough. Do we know that their Inertia Mass is proportional to their Gravitational Mass or that they fall under gravity with constant acceleration? The answer is, not by experiment, only by theory.

Is it the case that Newton's extraordinarily brilliant generalization about gravity has dazzled us even to this day? Einstein, according to that eminent scientific authority the Daily Mail, has recently exploded the Newtonian Law of Gravity, but even Einstein is probably so far hypnotized by Newton that he believes that equal masses of iron and of praesodymium sulphate will fall at equal rates.

The very nature of the difficulty, I think, suggests its solution. The experimental laws of mechanics worked out by Galileo and other early physicists, completed and summarized and built into a theoretical system by Newton have long been made the headstone of the corner of all Physics. Newton's ashlar may have been left rough hewn to be faced and polished up by mathematicians of to-day, but it has never been moved. If any of the builders took upon themselves to reject it they would bring the whole edifice tumbling about their ears.

If we took Galileo's experiments in isolation these objections might be plausible, but if they are considered in relation to the system of Newtonian mechanics towards which Galileo made some of the first and most important steps, they are seen to be trifling. The reason is not very easy to make clear, but is I think as follows. In the Newtonian mechanics we think we have somehow got very near the real ground plan of nature. We think that laws he enunciated are not only true but vital. I think the reason why the theories

of the early Greek Physicists—all their talk of Hot and Cold and Moist and Dry—sound so thin and futile to modern ears, is that we feel they had not really got to the bottom of things. It is absurd to say that they had not any basis of facts to go upon and were merely indulging in idle speculation. I am certain they had lots of facts. They were obviously most alert and observant men with enough brains each to make half-a-dozen Fellows of the Royal Society. What was wrong was that they had not hit upon the important sort of facts. With Archimedes' work on Statics we feel that he is getting towards the real point; the right ideas are beginning to form in his mind, his is "getting warm" as the children say. With Galileo, who knew and extended Archimedes' work, we feel that at last the real discovery is coming. Yet how few and rough his experiments in Mechanics were; a schoolboy could have done as well and as much. Of course he could, if he had thought of it! Those crude experiments of dropping weights from the tower of Pisa and rolling balls down inclined planes were nevertheless veritable gold mines of knowledge. All the experiments that have been done during the last ten years do not tell us as much about the world as his do. The ideas he was formulating were beginning to reveal something of the authentic ground plan of nature. It was not simply what he observed but the way in which he interpreted, selected and classified his experience that mattered. Others might have observed exactly the same facts and learnt nothing.

Now the ideas of which we find the seed in Archimedes, and which sprouted with Galileo, to blossom into the Newtonian mechanics were the notions of Inertia and Mass and of Force and Velocity.¹ If these

¹ I am quite aware that Energy is more fundamental than Force and that we have got beyond Newton's idea of Mass, and so on and so forth. But all this is mere decoration of the theme and does not affect the argument.

are really fundamental notions, ideas that tell us of the real stuff and substance of nature, then when we use them we are dealing with universal relations and characters of bodies, and if our still more primitive ideas from which we get the notion of bodies and substances and of the geometrical relations and properties of bodies are also fundamental; then we must conclude we are dealing with properties of bodies which are universal and do not vary from substance to substance. Evidence that the property of mass and the relation of masses of quite a few substances are similar is enough to enable us to extend our laws to any substance. It was in this way that Newton could find the same law in the fall of the apple and the rotation of the moon.

The first part of the Law established by Galileo was, as I have said already, that any freely falling body falls at the same rate under the same conditions irrespective of its weight. The justification for applying this literally to any body is that sufficient different kinds have been tried to show that variation of any relevant property makes no difference. The next step was to find what was the relation governing the increase in velocity of a falling body; if this could be done the problem of a body falling under gravity was solved. Galileo seems to have attacked the problem with complete confidence that the distance fallen was some simple function of the time. His simple-mindedness in this respect was that of the true experimental genius, for it showed him the possibility of settling the question by measurement. He made a number of measurements and it was obvious that the distance fallen was proportional to the square of the time. He thus established a very important law and introduced a new and valuable conception into science; that of an acceleration. I need not follow out his work any further, this much is enough for the present purpose.

As the result of these experiments there was no considerable disturbance of the common-sense classifica-

tion of things ; we may leave out of account his disproof of the Scholastic theory that bodies fell at a rate proportional to their weight. In the main his results were in the direction of the discovery of new facts, the clarification of existing ideas and the introduction of new ones. He had acquired from Archimedes the notion of Gravity as a force directed towards the centre of the Earth, the notion of Acceleration he invented (I take it) and the further notion of Mass was implicit in his work though perhaps not clearly expressed till Newton's time. With these notions Galileo was able to make and express observations and measurements in a way that was impossible without them, using the old ideas of weight, and the natural place of bodies and so on. All this essentially involved a revision of classification but more in the way of clearing up vague ideas and introducing new ones than of destroying old ones. The introduction of a new idea is a process of classification because the new idea provides a new class concept for the grouping of the particular facts of experience. It may happen that all the members of the new class were members of an old one, but a change will have been made if the new class is more adequately defined than the old one. It was in virtue of his new ideas that the simple and crude experiments of Galileo were instruments of such power. Without them the experiments could hardly have been made and if made would have been barren. The mere recording of observations will not result in the discovery of laws unless the right notions and methods of classification are there.

§ 13. In the present section I wish to give as briefly as possible some instances of how the existence of laws is dependent on the arrangement of the facts. The cases are drawn from physics rather than any other branch of science, because it may be plausibly maintained that there are no really simple laws relating to living things, whereas everybody admits that laws are to be found among any purely physical phenomena

provided we hunt for them diligently. What I wish to emphasize is the further fact that they must be hunted for in the right way or they will not appear. As Whewell said, the process of Induction depends for its success upon having the right Idea to start with.

Meteorology is based on the study of certain data collected at various times and places in a more or less systematic way. The data consists for the most part of measurements of Barometric Pressure, Wind Direction and Velocity, Air Temperature and Humidity, Solar Radiation, Rainfall and other phenomena connected with what is vaguely but conveniently called "Weather". Nobody doubts that there are Laws relating all these things, and that if the laws are known they will suffice adequately to describe the states and changes of the weather and to predict its course in the future. Further nobody doubts that the factors at work are those ordinarily dealt with in text books of Physics—"Heat", "Light", "Properties of Matter", and so on. Ultimately the whole set of phenomena are considered as capable of being described (or "explained", to use a popular but misleading expression) in terms of the general laws of physics, *e.g.*, the movements of the air, we call Wind, in terms of the laws of Mechanics; the precipitation of moisture in terms of the thermo-dynamic properties of gases, and so on. But the abstract general laws of Physics can only be applied if the meteorological data have first of all been arranged and displayed orderly in the form of special meteorological laws. Now at first sight meteorological data are about as chaotic as anything very well can be, and it would seem to be as profitable to look for simple laws among them, as to look for snakes in Iceland. But there is a simple device which helps to clear up the confusion; this is the well-known method of displaying the variation of the meteorological factors at a given time from place to place by means of a Synoptic Chart. More particularly, from the geographical distribution of barometric pressure

lines of equal pressure (Isobars) can be drawn on the map. The study of a series of such charts drawn for successive times at once reveals certain regularities. There is an intimate relation between the arrangement of the Isobars and the velocity and direction of the wind; there are certain characteristic distributions of Isobars which correspond to certain well defined types of weather; and lastly there is a tendency for the system of winds and pressure distribution to move across the map as an individual whole for hours, days, or even weeks in a fairly regular way. In fact the whole method of weather forecasting in use at present is based on the study of synoptic charts and the classification of types of weather that they have made possible. This process of plotting out the instantaneous distribution of the meteorological elements over a region of the earth's surface and then watching the way it changes with lapse of time is not in any sense a method of generalizing from the data but merely of classifying the data. The facts are just the same as they were before the chart was drawn, the only difference that has been made is in the arrangement of them. It is even more instructive to examine the defects in this method of recording the data.

If we look at a chart showing a typical "cyclonic" distribution of isobars it shows the wind blowing round the centre of the cyclone in a counter clockwise direction, in the Northern Hemisphere, the direction of the wind being nearly tangential to the isobars. When we remember that the whole cyclone will be moving bodily in an easterly direction sometimes with considerable speed, and further that the air temperature in different parts of it is very different, it is seen to be unlikely that any given mass of air is actually rotating round the cyclonic centre. It is more probable that the cyclonic centre is a fictitious entity, and that the air that at any given moment is blowing tangentially round it has moved towards it and will afterwards move away, and that as the cyclone moves from west to east it does not

actually carry the air with it. Now although there is nothing actually visible on a series of synoptic charts to show where the air has come from and where it is going to, and so far the charts are misleading, the necessary data for calculating the actual paths of the air currents over the Earth's surface are all there, and only require arranging and working out. This has been done in a number of cases and a beginning made towards the real study of the dynamics of air currents, and even the few published results indicate that this new method of classifying the data as to air movements will be fruitful for the discovery of new laws.¹

Whereas the synoptic chart introduces law and order among barometric observations, temperature observations plotted out in the same way remain rather chaotic and practically no simple generalizations are possible, though a study of the actual air trajectories affords some help. One reason for the failure of synoptic charts in respect to temperature data is the very large diurnal range of temperature at any one spot which "affects a synchronous chart in a very special manner. The region of the daily weather map of the Meteorological Office extends from the Azores in Long. 28° W. to North Sweden in Long. 25° E. and the difference in longitude implies a large difference of local time and therefore of local temperature. Thus when it is sunrise at Greenwich it is two hours before sunrise at the Azores and nearly two hours after sunrise at Haparanda. There are consequent differences of temperature which make a temperature chart for 7 A.M. a structure of very complicated meaning".² Possibly a synoptic chart based not on simultaneous observations but on observations at corresponding local times would display temperature variations in a more systematic manner, but such a chart

¹ *The Life History of Surface Air Currents* by Shaw and Lempfert, 1906, Met Office Report, No. 174, gives a full account of the work and there is an excellent summary in Ch. VII of Sir Napier Shaw's book *Forecasting the Weather* (1913).

² Shaw, *Forecasting the Weather*, pp. 80 and 81.

would turn the representation of all other meteorological elements into a hopeless chaos. Generally speaking the kind of classification which enables the laws of pressure distribution to be discovered is not that which displays the laws of temperature distribution, whatever they may be.

Tides and Tidal Streams in narrow waters provide an excellent example of extremely confused phenomena which are admitted to be due ultimately to the operation of simple dynamical laws, but which are very difficult to express in the form of laws at all. Without going in any detail into this difficult but interesting subject one case where a chaos of facts can be turned into comparative order may be given very briefly. When a tidal wave enters shoal water, as for instance the Atlantic wave entering the English Channel, part of the energy of the wave is transformed into a horizontal streaming movement. It would naturally be expected that on the flood tide the stream would set in the direction of movement of the crest of the wave and on the ebb in the opposite direction, as indeed happens at the mouth of a tidal estuary. But this state of affairs is only found at some parts of the Channel; at others the stream actually runs in the opposite directions and at others it changes its direction at some time intermediate between high and low water. That is to say though the times of high water at any point and the direction of the tidal stream at any time at that point can be predicted by the usual methods, there seems to be no way of correlating the ebb and flow of the tide at any place and the tidal stream, or of correlating these processes at different places. If all the data were tabulated, undoubtedly all the information would be there, but in such a chaotic form that it would be a long and laborious business for the navigator to work out his course. This chaos of events can be arranged in a comparatively orderly fashion, as regards the tidal streams, by referring them all, not to local times of

high water, but to the time of high water at Dover. Then it is seen that at High Water and Low Water at Dover the streams in practically the whole Channel are slack. As the tide begins to flow or ebb at Dover so the water in nearly the whole of the fairway of the Channel sets inwards or outwards, towards or away from the Straits; at the same time the water in the southern part of the North Sea sets towards or away from Dover. There is a slight complication in the stream in the straits between the area occupied by the Channel and North Sea streams. The water in this intermediate area at the beginning of flow or ebb all sets towards the North Sea end, but gradually, the line of junction or separation moves easterly until at the end of the flow or ebb all the water is running with the Channel Stream.¹¹ In this way, while we have added no fresh data, simply by rearranging the facts we can get from a confused mass of facts to a comparatively orderly one, among which laws are to be found, and such that the description can be couched in general terms.

I have chosen these rather out of the way cases as they show that the mere knowledge that there are simple laws at the bottom of events is no earthly help in itself towards getting the description of the events into such a shape that these simple (and of course general and abstract) laws can be applied. Until the preliminary detailed work of the Meteorologists has been accomplished it was about as helpful to believe that the wind was due to simple physical causes, like the Earth's rotation and the heating of the atmosphere by the sun, as to believe that the "wind bloweth whithersoever it listeth." In either case we had to find out first how the wind actually did blow on particular occasions and to

¹¹ Some idea of the nature of the raw data can be got from the detailed information in the Admiralty publication, *Tide Tables for British and Irish Ports and Tides and Tidal Streams round the British Isles*. The general scheme is detailed in the latter work and also in *The Channel Pilot*.

correlate that if we could with something else. [However much we may be convinced that an apparent chaos is a cosmos at bottom, if we only knew it, we still have to use experience and the primitive process of trial and error, or our conviction is vain. Moreover, the most vital part of this preliminary process is how we classify our facts; it is that that in the first stages makes the difference between chaos and order, not any general belief in the Reign of Law.]

It might be argued that all the relevant data for a science of Chemistry and a good part of Physics are to be found in Landolt Bornstein's *Physikalisch-Chemischen Tabellen*. That is undoubtedly true for anybody who already knows something of the relevant sciences. But nobody who was not a genius could learn any science by studying tables of constants, simply because, although the tables give him data, he has not got the necessary apparatus of ideas with which to arrange them.

Lastly let us consider the problem investigated by Lord Rayleigh when he was measuring the densities of gases.¹ In the late eighties he made some very accurate determinations of the ratio of the densities of Hydrogen and Oxygen, which ratio is one of the most important and fundamental constants in Chemistry. Later he went on to determine the absolute densities of various gases. For Air and Oxygen he obtained results of great accuracy. Then, suspecting no evil, he went on to Nitrogen. But here he found his results were not concordant to the extent his previous work led him to expect. After making a number of determinations as carefully as possible he found that the average density of Nitrogen obtained from Air, by removal of Oxygen and the other gases, was about one-half per cent. more than the average density of Nitrogen obtained by the decomposition of Nitrogen compounds. The difference

¹ References *Proc Roy Soc* 1888, 1892, 1893, 1894, 1895 There is a critical summary of previous gas density work by Guye in *Chemical News*, 1918, Vol XCVII, pp 53 and 68

was small and for many purposes negligible and had not in fact been noticed by previous workers. But still it was clearly unsatisfactory to have to acknowledge the existence of two gases identical in all respects except for this small difference in density. If this discrepancy had been discovered at the present day, all the wise men would have said "Ah, yes; of course; Isotopes of Nitrogen. How interesting!" Fortunately Rayleigh did not do this, but enquired instead into whether some or all of his nitrogen might not be contaminated with an impurity. After seeking in vain for some noticeable impurity in his nitrogen he remembered an almost forgotten observation of Cavendish. Cavendish, with that infinite capacity for taking pains which in his case amounted to genius, had succeeded in showing that the nitrogen of air could be made to combine with oxygen, except for a small residue "not more than 120th of the whole." It occurred to Rayleigh that this residual gas might be the impurity he was looking for and he repeated Cavendish's experiment. Using an easier and quicker method than Cavendish he succeeded in getting all the "nitrogen" of a sample of air to combine except for a small fraction which had a higher density and was in fact a hitherto unknown gas. He then showed that the nitrogen from air after purification from this gas has the same density as nitrogen obtained from a compound, and the science of chemistry was vindicated. The subsequent history of his new gas I need not retail, it is well known.

From our point of view what Rayleigh had done was to reclassify the bodies formerly called nitrogen into "nitrogen" and "nitrogen plus argon". By so doing he was able to show the correctness of the old empirical law that bodies with a number of specific properties in common have all specific properties in common. But we know that there is no a priori necessity for this law and we even know that it is not always true. This does not detract from the value of Rayleigh's work, because

he was not guided by the belief that this law must be true (at least I think not) but by the instinct of the true experimentalist that where there is an anomaly there is something to be discovered.¹ The successful discoverer is the man who believes in his observations rather than in the theories he has learnt from others or even those he has made up for himself. There are many other morals to be drawn from this classical research.¹ One is that an investigator never knows what he is going to find. Rayleigh was apparently doing the very dullest sort of work—redetermining a constant of one of the most carefully investigated substances, to get another significant figure in. It will be remembered that when Saul went out to look for his father's asses he found a kingdom.

§ 14. I have argued that our general knowledge of the external world as expressed by laws of nature, is a product of the interaction of two processes, selection from and classification of what is experienced and the discovery of laws relating the classes. The two processes are linked together and in fact developed simultaneously, so that we adjust our classification in order to obtain general laws and apparently believe in our laws because of the excellence of our classification. It sounds as though there might be a vicious circle involved in this process. That is to say the laws are not laws of nature, but identities of definition or relations among the class concepts which are deducible from them, and hold good by definition whatever the facts of the case.

Against this we may point to the fact that laws are as a matter of fact constantly turning out to be false. Suppose for a moment that Lagrange's equations were deducible from the definitions of the terms involved, it still remains to discover by observation whether there are things whose properties can be adequately described in these terms and whether the particular observed values of the terms satisfy the equations.¹ All this involves a purely empirical process that cannot be turned

into a deductive one. It might be interesting to the pure mathematician to study Lagrange's equations for the sake of the form of the functions, but he would never get an inkling that these were the Laws of Motion, unless he carried out an experiment, made his symbols stand for measurable quantities and then made the measurements. Given any body of experience we are at liberty to classify what we experience either as a system of the form $p_1, p_2, p_3, \dots, p_n$ or of the form q_1, q_2, \dots, q_n or r_1, r_2, \dots, r_n , or of any other form just as we like. Then if we take any of these systems P, Q , or R , or any other, we may either observe or deduce the relations holding among the classes and functions. If we find the relations by deduction we must then make the necessary observations to see if they hold, while if we find the relations empirically we must see whether the relation is compatible with the form of the system (what is described as "giving a physical meaning to an empirical formula"). In either case the result is exactly the same logically, the difference is merely in the historical order of the operations. And in any case from our systems, P, Q, R , we select the one giving the most manageable relations. It is hard to see how there can be any vicious circle provided that the relevant observations are honestly and genuinely made and applied in the correct way. The fact that we may find our laws and our classifications imperfect and change about from one to the other according to circumstances does not make any difference. The further fact that in a favourable case one observation may establish a hundred laws does not imply that ninety-nine could have been established without any observation at all.)

It may be assumed that the Phase Rule of Willard Gibbs proceeds logically from a few of the most general and abstract notions that we can possess on the subject of matter and energy; and it is conceivable that the principle would be perfectly intelligible to a being who had no experience corresponding to our experience of

the behaviour of material objects. But it still remains to see whether there are systems which do really behave in the way that the Phase Rule requires. It might happen that no actual aggregate of bodies ever was in a condition approaching the condition of thermodynamic equilibrium upon which the deduction of the Rule depends. If this were so, the Rule would still be just as true, but we could never apply it. It is notorious that the scientific world took no notice of Gibbs' work for many years, not until the Dutch chemist Roozeboom took it up. This is usually attributed to the difficulty of following his argument and to the obscurity of his medium of publication—the Transactions of the Connecticut Academy. But there is, I think, a further explanation. Nobody now knows what the learned men of Connecticut thought about "Heterogeneous Equilibrium" in the seventies, nor what the industrious Teutonic abstractors who must have at least looked at the paper, thought. But it seems likely that nobody at the time knew that the Phase Rule was a law of nature. Gibbs gives no hint that this particular deduction can be applied to any concrete case; it might be a proposition in pure mathematics as far as he is concerned. But when Roozeboom went into his laboratory and showed that things did really behave as Gibbs said, everybody was full of admiration. This story is sometimes told as illustrating the obtuseness of ordinary scientific men, who are not worthy of the pearls that lonely geniuses like Gibbs cast about before them. Is it not the case rather, that the general instinct of men of science is quite sound? What they want is an experiment; they want somebody actually to go and measure something and draw a curve. I should not wish for a moment to be supposed to be decrying Gibbs, who was probably one of the dozen or so men of supreme genius, but I think due honour ought to be paid to the man who first realized that the sort of things that Gibbs had deduced from first principles were also the sort of

things that can be applied in the laboratory or the factory ; that Gibbs in other words was deducing a Law of Nature, something that could actually be used to help people to crystallize Potassium Chloride from a solution of Carnellite. Lots of people can deduce things from first principles, but they are not laws of nature.

This argument does not meet the whole of the objection raised. It may still be said that our belief in laws rests on no firm basis if laws are by definition the sort of relations that hold among natural kinds, and natural kinds are the sort of classes among which there are laws. This argument is in a sense unanswerable, so far as I can see. General beliefs not obtained deductively but based on matters of facts have no rigid ground. An act of faith is necessary at some point or other. This must be acknowledged. Still, a man is free to believe anything he likes as long as his belief does not bring him into collision with the police ; and there seems to be no ground for objecting to acts of faith provided two restrictions are recognized. First, that the amount of "pure" faith is kept within the narrowest possible limits. Second, that the act of faith is always made tentatively and as liable to revision.

CHAPTER IV

The Validity of Laws

§ 15. THE questions discussed in the previous Chapter have been concerned with the relations between the laws that can be discovered and the classification of the constituents of the external world ; no serious attempt has yet been made to find what are the rational grounds, if any, for believing in the validity of natural laws. That is to say the logical status of the process of induction still remains to be discussed. Keynes in his Treatise on Probability (Chapter XXIII) remarks with astonishment how few are the books dealing with Induction. He ought, I think, to have expressed some gratitude to the multitude of authors who have refrained from writing on the subject. As the result of their self-denying ordinance the student of the subject can read a small number of quite good books instead of being faced, as usually happens, with the task of turning over vast dust-heaps, excreta of the human mind, in the hope of finding here and there something of value. As Keynes says most logicians content themselves "with the easy task of criticizing Mill or the more difficult one of following him." Meanwhile Hume, who pooh-poohed the whole process, remains master of the field.

Keynes¹ own treatment of the subject is candid, lucid and masterly ; and yet at the end of it all we are left almost exactly where we were before. That is to say we can find no reasonable ground for believing in the results of Induction, but we go on believing just

¹ Most of what follows is due to Part III of his *Treatise* and to Broad (*Mind*, Oct 1918 and Jan 1920), whose discussion is on similar lines

as much as we ever did. Nowhere do sceptical arguments have so much force or so little effect.

There was one very important mistake made by Bacon in his treatment of the subject and repeated by Mill.¹ These authors expected inductive reasoning to produce certainty and thought that an induction that was not certain was worthless, so that if it turned out to be wrong it cannot have been legitimately inferred from the previous evidence. This, of course, is not the case; an inductive argument is no more than probable, that is to say, relative to a certain body of evidence it is rational but not conclusive. The fact that fresh evidence contradicts it does not imply that it was not rational to believe in it, lacking that evidence. There is a further modification of our claims for inductive generalization that might be made. 'We may consider as Keynes points out² a generalization to mean either "It is probable that all ϕ are f ", or "It is probable that any given ϕ is f ." Certainly many, if not all, our generalizations are of the latter kind; they are what Keynes calls Inductive Correlations as opposed to Universal Inductions. A Universal Induction is proved false by one negative instance; with an Inductive Correlation a negative instance will produce only a small change in the degree of probability attached to it.'

But even if we moderate our claims for induction in this kind of way it does not really solve the problem. The logical problem of the validity of Inductive Correlations is precisely similar in all essentials, but much more complicated than that of the validity of Universal Induction. The chief difference, as Keynes points out,³ is that the latter is concerned with the characteristics of individual propositions and the former with series or sets of propositions. This difference does not make the results of logical analysis any more satisfactory or place correlation in a stronger position. In fact it is better to consider the argument for the case of Universal Induction

. ¹ See Keynes, *loc. cit.* p. 267

² *loc. cit.* p. 259

³ *loc. cit.* p. 406

as being the type of the other, and possessing the advantage of simplicity.

Some writers on Induction have used the Rule of Succession by which a very high degree of probability is assigned to general assertions about the next of a series of events or any one of a series, so giving apparent validity to Inductive Correlation¹. But Keynes² argues, correctly I think, that the Rule of Succession is a fallacy. Certainly, although its results sound plausible when long series of events are considered, they are absurd when applied to events that have only been observed once or twice or not at all. It suffers from the further drawback that the conclusion is inconsistent with the premises from which it is deduced.

A probable belief, as Keynes says, may be reasonable because either (1) it is the best open to us under the circumstances, or (2) it is more probable than not. It is in sense (2) that we *want* to show that Inductions are probable, but it may be only in sense (1) that they are justified. That is to say we may be justified in believing in the truth of a natural law because we can find no tolerable alternative, although we can never have any reason for thinking that it is more likely to be true than false or even that its probability has any finite value, however small. It is not difficult to justify our belief in natural laws in this sense, but it is rather a dismal conclusion. It seems to contradict, too, our common-sense belief that some inductions are more probable than others and that the best are infinitely probable, that is to say that their probability differs from certainty by less than any assignable amount. But, of

¹ Broad (*Mind*, N S, Vol 27, p 394) has made use of this rule, but it is not essential to his argument.

² *loc. cit.* p 375 The formula states that if there are conditions which as far as we know may or may not produce an event, and under these conditions it has happened m times and failed n times, the probability of its happening when they next occur is $\frac{m+1}{m+n+2}$. If the event has never failed this reduces to $\frac{m+1}{m+2}$.

course, it may simply be that this common-sense belief is only the most reasonable open to us and it may not itself have any finite probability.

Our ordinary prejudices make us want to get in certainty somewhere. We are prepared to give up the idea that inductions are certain, but we want certainty that they have a finite probability, or at least a finite probability that they have a finite probability. Once we have started like this we must go on. The prospect of an infinite regress of probables even if they are all finite is no better than that of an infinitesimal probability to begin with. The fact is of course that you cannot get certainty out of ignorance, but there is an immense temptation to try and do it. All the attempts to do it, are like the efforts of more or less competent conjurors to take rabbits out of top-hats. Sometimes we can see how it is done, sometimes we cannot. Even if we cannot, we are not convinced the rabbit was there all the time. Since man is acquainted with a finite number of things in an infinite and infinitely diversified universe what can he expect? His feet are compelled, as Santayana says, to move forwards, whilst his eyes see only backwards. He is always moving out of the warm circle of familiar things into the unknown and longs for certainty and prophetic insight where only reasonable conjecture is possible.

The most reasonable ground, on which a higher validity has been claimed for Induction, is a belief that in some discoverable respect the Universe is not infinite or not infinitely diversified; a doctrine that will have to be examined later. Some thinkers, however, impressed with the difficulties of Induction, have thought there might be some short cut to knowledge by way of deduction only, avoiding not only the uncertainty of Inductive methods, but all the toil of examining the facts. This illusion has died hard, if it is really dead. Quite apart from the fact that deductive reasoning by itself can only tell us about the relations of propositions

and not about things, there is another flaw in this argument. The supposed certainty and precision of deductive reasoning is illusory unless the functions and limitations of the process are kept in mind. The most that deduction can establish is that if certain axioms are true certain results follow, but whether the premises or the conclusions actually are true is an empirical generalization. Our faith in the validity of the process by which conclusions are obtained from premises depends upon their both being true as a matter of fact. If we have no empirical criterion but take the axioms simply as hypotheses, then we are dependent for our belief in the cogency of the reasoning on our inability to find a flaw in the argument, and are at the mercy of anybody cleverer than ourselves who can point one out. It is not unknown in the history of Mathematics for proofs that have long been accepted as correct to be shown to be fallacious after all. In the last resort we are always brought up against an empirical generalization of some sort. If we are to reason about or know anything we must be satisfied to accept as true what does not appear to be false }

§ 16. Before discussing the validity of the process of induction any further it will be as well to give as briefly as possible the results of Keynes' analysis of the matter.¹ A generalization, he says, is a statement that all of a certain definable class of propositions are true. If $f(x)$ is true for all values of x for which $\phi(x)$ is true, that is a generalization about ϕ and f and may be written $g(\phi, f)$. An instance of the generalization will be $\phi(a), f(a)$. If anything is true of a number of objects so that they satisfy the same propositional function there is said to be an *analogy* between them. The generalization $g(\phi, f)$ asserts that one analogy ϕ is always accompanied by another f . The *positive analogy* is the set of propositional functions which are satisfied by the objects in question. As the positive

¹ loc. cit., chs XIX and XX

analogy measures the resemblances, the *negative analogy* measures the differences, and is the set of functions such that each is satisfied by at least one of the objects but is not satisfied by at least one other. This may be called ϕ^1 . In addition to negative analogies proper which are true of one instance only there may be sub-analogies which are true of a set of instances but not of all. There are, in addition, superfluous positive analogies ϕ_1 , in which all the instances resemble one another but which are not included in the generalization.

Apart from the accumulation of evidence there will be some prior probability possessed by the generalization which will depend, among other things, upon the nature of ϕ and f . The value of this can be increased principally :—

- (1) By reducing the resemblance ϕ_1 , known to be common to all instances but ignored as unessential.
- (2) By increasing the difference ϕ^1 known to exist between instances.
- (3) By diminishing the sub-analogies known to be common to some instances and not known to be false of any.

The increase can be obtained either by obtaining fresh instances or by re-examining those we already have.

The value of mere increase in the number of instances, Keynes urges, is easily over-rated and is chiefly valuable in so far as the analogies are weak or unexamined. An examination of the analogies, which is the method of exact science is certain to increase the probability of the generalization. Increase in the number of instances may increase the probability, but does not do so of necessity. The tendency of fresh instances to increase the probability of the generalization depends upon the width of the field from which they are drawn and their variety.

Keynes shows that by mere accumulation of instances without examination of analogies (Pure Induction) the probability of a generalization may be increased so as to approach unity as a limit under certain conditions. In particular for the accumulation of instances to increase the probability of a generalization, the probability of the whole set of them in the absence of the generalization must be small compared to the prior probability of the generalization itself. So that unless the generalization has some finite probability the accumulation of favourable instances is no help. It must be understood, of course, that in saying a generalization has a finite probability it is not implied that a numerical value can be assigned, but that it can be defined as having a greater probability than some proposition to which a finite numerical probability can be assigned.

The argument that the instances in order to support the generalization must have a low degree of probability on the assumption that generalization is false, is quite in accordance with common-sense belief. The point is well illustrated by Kirchhoff's argument for identifying the dark lines in the Solar Spectrum with the absorption bands of terrestrial substances to whose position they very closely approximate. He argued that assuming a random distribution of dark lines in the solar spectrum the probability of such a close coincidence as was actually obtained with a large number of lines was excessively small. The weak point of this particular argument is that a random distribution is not the sole alternative to the hypothesis of the identity of the causes of the two kinds of bands. The best that can be said is that no other alternative has been suggested. Moreover, there are so many cases of events, which are excessively improbable in relation to all human knowledge, actually happening, that arguments from improbability must be treated with caution. Kirchhoff's whole argument really rests upon the assumption that

what goes on in the sun is likely to be the same, *mutatis mutandis*, as what goes on on the earth. Given this hypothesis the whole argument is quite reasonable, because this assumption gives the special hypothesis as to the absorption bands a fair probability *a priori*.

This brings us back to the most important and difficult point in the whole question, that a hypothesis must have some probability prior to the observation of any instance that supports it. This prior probability must turn, not apparently upon matters of fact, but on some other generalization or upon the character of the analogy concerned. Jevon's assumption that in the absence of evidence any hypothesis is as likely as not may be dismissed at once, as leading to contradictions. Before discussing the possible solutions of this question there is a minor point to be disposed of.

It is sometimes suggested that a simple generalization has a greater prior probability than a complex one.¹ The complexity of a generalization is measured by the number of logically independent propositions into which it can be analysed. There are very often obvious pragmatic reasons for preferring a simple to a complex hypothesis as the simple one is usually easier to deal with. If other things were equal, the pragmatic reasons would determine us to choose to use a simple generalization in preference to a complex one but that would not settle the logical question. The arguments of Broad² and the more complete analysis by Keynes³ show that there is something but not much in the belief. Broad argues simply that the probability

¹ This has been put forward as an important methodological principle in a recent paper in the *Philosophical Magazine* (Vol. 42, p. 369, 1921) by Drs D. Wrinch and H. Jeffreys. It is combined with the curious doctrine that all laws of Physics can be expressed as differential equations, a doctrine that is offensive to upholders of the Quantum Theory and suffers from an even more serious and obvious difficulty. Differential equations can only be stated as inferences from generalizations that are not capable of being so stated themselves.

² loc. cit. p. 402

³ loc. cit. p. 244

of a hypothesis relative to any given body of evidence is the product of the probabilities of its constituent propositions relative to the same evidence, which must all be less than unity. There is a probability that the product of a smaller number of proper fractions will be larger than the product of a larger number. Hence simple generalizations have a tendency to be more probable than complex ones. Keynes shows that as regards the consequences of a generalization an increase in complexity cannot make it more probable and may make it less probable, and on the other hand an increase in the complexity of the conditions cannot make it less probable and may make it more probable. That is to say an increase in the number of independent characteristics that are specified in order to determine what events are to be included as instances may increase and will not diminish the probability of the generalization about them. On the other hand an increase in the number of independent characteristics which an event included as an instance is inferred to have may diminish and cannot increase the probability. Thus we are left with the watery conclusion that the more complex generalizations are not likely to be so probable as the less complex, but we have not proved anything. Moreover it does not follow that the very simplest generalization possible has any finite prior probability, which is the important point.

§ 17. The favourite device for saving the face of Induction is the Principle of the Uniformity of Nature. I am not aware when the Principle was first introduced to an admiring world; but, as it has a pleasantly Victorian flavour about it, I am inclined to conjecture that it made its debut at the Exhibition of 1851, when it was awarded a Silver Medal. Comic though the Principle may be in some aspects, it nevertheless needs to be discussed very seriously. The objections to it may be put quite briefly; in the only meaning that can be assigned to it that is not palpably false,

it does not fulfil the function it is intended to. The argument in its favour is that when you have said all you can against it you are apparently compelled to acknowledge some assumption about the nature of the external world which comes to very nearly the same thing.

If the Principle of Uniformity be taken to mean that the course of events in nature is uniform it is certainly false, as anybody can see for himself. That it is ever taken to be true is due to the fact that people seldom trouble to observe things for themselves, but are content with any second-hand theory that comes their way, and further that civilized beings are so accustomed to the uniformities produced by mechanical means that they imagine them to be natural.

If the course of nature were uniform there would be no need of Science. Such regularities in events as are to be observed apart from the theories, instruments, and mechanical aids of science are precarious and few. The generalizations from them are of little scope and doubtful application ; they are liable to frequent exceptions. Nature does not wear her heart on her sleeve for daws to peck at.

Consider the difficulty of teaching people to comprehend the laws of nature as conceived by contemporary science ; the immense elaboration of apparatus and technical skill in using it that is required to demonstrate them by experiment. Teachers of science are always bewailing the fact that the major portion of scientific theory has to be taught dogmatically and accepted on authority, and that only minor points can be observed and demonstrated by the pupil himself. It is not at all an easy thing to demonstrate by experiment the Law of Conservation of Energy or of Mass, the Law of Constant Proportions, or the value of the Mechanical Equivalent of Heat or any important physical constant. The unskilled experimenter who tries these things is not likely to finish with any

increased belief in the Principle of Uniformity or in anything else¹

We must dismiss the notion that the course of events in nature is uniform as a delusion; but it does not follow that the Principle of Uniformity is therefore false or that nature is not orderly through and through. It only means that the order is not immediately apparent but has to be looked for. The Principle of Uniformity is usually taken to mean just this, that if we look for order in nature in the correct way we shall find it. This is a very pious sentiment and may very well be true, but does it give us the least help in actually discovering laws or in establishing them as the correct ones? Nature might be orderly through and through, and the reign of law absolute, and yet the laws might be so complex and so difficult to discover that as far as human beings were concerned there might as well be complete chaos. As Russell has pointed out,² any collocation of entities in space and time would have some laws, in the sense that there would be a set of equations expressing their distribution and mutual relations; but these laws might be of any degree of complexity or simplicity. The remarkable thing, therefore, about the existing world is not that there are laws but that they have been discovered and that they are comparatively simple. "But it is just this characteristic of simplicity in the laws of nature hitherto discovered which it would be fallacious to generalize, for it is obvious that this simplicity has been a part cause of their discovery and can, therefore, give no ground for the supposition that the other undiscovered laws are equally simple." The Principle of Uniformity therefore needs to be modified so as to mean that there are

¹ It must be remembered that Astronomy was the first science to develop and the first to become exact, and this fact has coloured all subsequent thought. What plausibility the Principle of Uniformity has is due to the fact that among the heavenly bodies mere observation, without active interference in the course of events, reveals completely orderly behaviour.

² *Mysticism and Logic*, p. 101

laws of nature which are simple and discoverable, but in this form is even more impotent as an aid to induction.

§ 18. Broad has attempted a formal analysis of the Principle of Uniformity¹. Stated as he states it in symbolic form it looks rather sheepish, but his treatment cannot be dismissed as mere caricature because it does seem to express quite correctly at least part of what Mill meant by it. His statement may be paraphrased in terms of classes, with some loss of generality, as follows: "If a member of a class L is a member of M also, there is another class N, such that any member of L and M is a member of N". Or we may say: "If a and b are members of class L, and a is an M, but b is not, then there are classes N and O of which a and b respectively are members and such that no member of N is a member of O". If these propositions are considered as applicable to any class they are false. There are people who are inhabitants of Bloomsbury and are bald, but it does not follow that they have any other characters in common, except such as are implied by being an inhabitant of Bloomsbury or being bald, such for example as being human or wearing clothes. On the other hand there are classes of which we believe these propositions to be true and those are the type of class among which inductions are supposed to hold. But even if we admit the Principle to be true in the restricted sense, that there are classes among which these relations hold, the principle does not help us to find them or know that we have got them when we think we have found them. We may think we have got hold of classes L and M, but how are we to find class O or know when we have got it?

These propositions stated by Broad (I refuse to call them by the insulting nickname he attaches to them) make no explicit reference to the nature of the universe as a whole, and that is to their credit, but it is clear that

¹ *Mind*, N.S., Vol. XXIX, p 11 (1920).

their claim to be considered true must be derived from some theory as to the nature of the universe.

Keynes gives rather a different interpretation of the Principle of Uniformity.¹ He considers that it means that mere position in space and time is irrelevant to generalizations which have no reference in themselves to particular spatio-temporal positions, but elsewhere he mentions other principles which may quite properly be considered as included in the Principle of Uniformity as usually conceived. The most important aspect of the doctrine of the irrelevancy of spatio-temporal position is the belief that the position of an event within or without the sphere of our present and past experience is irrelevant to generalizations about it. If we did not believe this, all inference to future events or to any events not actually experienced would involve an imperfect analogy simply because they were not experienced, whereas the events on which the generalization was based had been. This is quite definitely a theory about the nature of space and time and is moreover one that is open to criticism by ordinary methods of science. We are not compelled to start by making any assumptions as to the nature of those parts of space that are not experienced, but it is conceivable that examination of the properties of the limited region, within present and past experience might lead us to consider it illegitimate to make inferences as to the region outside or it might lead us to think it legitimate. I am not going to discuss the question of the truth of this theory, I am merely interested in pointing out what sort of theory it is. The principle of uniformity as ordinarily conceived is a metaphysical theory, it is a theory as to the nature of the Universe as a whole to be taken or rejected as it stands and only to a very limited extent open to criticism by scientific procedure. Considered as an inference from the results of scientific investigation it is of such wide scope as to be obviously illegitimate, considered as an axiom pre-

¹ loc. cit. p 226

supposed in scientific investigation it is outside the possibility of criticism, again because its scope is too wide, not because such axioms are immune from all criticism.

Keynes in his discussion of the validity of induction finds himself compelled to consider two further principles,¹ that of the Limitation of Independent Variety and that of Atomic Uniformity; principles also recognized by Broad in his discussion of the subject. The first is the theory that in certain respects Nature is not infinitely variable but is built up of a limited number of constituents about which we are as a matter of fact supposed to know something. The second is that many, if not all processes can be considered as compounded of small changes according to simple mathematical laws. "The system of the natural universe must consist, if this kind of assumption is warranted of bodies which we may term (without any implication as to their size being conveyed thereby) *legal atoms*, such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state. . . . Each atom can, according to this theory, be treated as a separate cause and does not enter into different organic combinations in each of which it is regulated by different laws." These theories even more obviously than the other are such as would be recognized by physicists as specific theories about those parts of the external world which are dealt with by their methods and they would be criticized by them exactly in the same way as any other theory. I am assuming a very liberal minded physicist and one who never swallows theories or assumptions whole and unexamined. Many of the more conservative kind would think Atomic Uniformity and the Limitation of Independent Variety too obvious to be worth thinking about.

¹ *loc. cit.*, ch XXII, and last section of ch XXI.

The conclusion we must come to, then, is that inductive processes cannot be defended on logical grounds only but material considerations must be brought in. We must appeal not to the nature of propositions but to the nature of the external world. The difficulty is to avoid making this appeal appear to be illegitimate and impotent. Mill appealed to the Principles of Uniformity and of Causation, and we criticize him because these material principles are too general and too vague to be useful or legitimate. They go beyond the proper bounds of scientific theory so that their standing is doubtful and they are not found to be of any help for the solution of any particular problem. The Principle of Causation, it may be mentioned in parenthesis, has not been referred to before because it introduces no fresh element of importance and has fallen into even greater disfavour than Uniformity. It has of late years been very severely handled by philosophical critics, particularly by Russell, and scientific men have simply forgotten about it.

What we must appeal to to justify inductive reasoning is simply the general body of scientific theory. Keynes selects certain parts or aspects of the theory which seem specially relevant to his purpose, but treated apart from the rest of the theory of which they form part they are not specially plausible. By themselves there is no particular reason for thinking them true or false. When we confine our attention to a perfectly general and abstract statement of the process of induction apart from the investigation of any special problem we are confined to an appeal to perfectly general and abstract principles to support it, and this we have seen leads to difficulties. The only legitimate appeal is to the nature of the special parts of the world that are being investigated. The general case always looks like attempting to get knowledge out of ignorance.

With Mill we must explicitly reject any "principle which, antecedently to any verification by experience,

we are compelled by the constitution of our thinking faculty to assume as true." Mill's healthy British scorn for any such foreign importations is an attitude I approve of, but it must be admitted that there are others less strong-minded. To these we must reply, that in a free country everybody is entitled to what metaphysical system he likes, provided he does not bother other people with it, and does not think that it has any logical connection with the assumptions or results of science. That is to say we must be able to justify our belief in the validity of scientific methods without assuming any system of metaphysics to be true or false. Any assumptions that are made for purposes of scientific investigation must be assumed as correct until they lead to contradictory results, then they must be corrected, but it is all a technical matter that does not concern metaphysicians acting in their official capacity. If some or all of the results of science are due to the constitution of the human mind we must just put up with the consequences until some non-human mind can explain to us how things really are.

There are two other valuable arguments we can get from Mill. No principle such as Uniformity, or any substitute for it, can validate induction in the sense of standing as a universal major premiss to an inductive syllogism. Inductive reasoning cannot be twisted into syllogistic form. Lastly, whatever theories are used to support an inductive argument must be themselves the results of induction, as we have seen all other possibilities are excluded. Mill admitted frankly that the Principle of Uniformity must be an inductive generalization. He probably realized perfectly well that all the clever critics would seize on this as self contradictory, but of course such an attitude if maintained with due reservations is not necessarily self contradictory, as Keynes argues.¹ That is to say if we can somehow find reasons *a priori* that give our fundamental

¹ *loc. cit.* p 259.

hypothesis a finite probability and the evidence that accumulates increases the probability of the hypothesis above its original value we can in future treat it as possessing this enhanced value, and further we can treat our accumulated evidence as supporting our original a priori arguments. This is in accordance with ordinary scientific procedure.

Our general knowledge of some particular region we wish to investigate gives us a small finite probability that some generalization is true so that we start off with a hypothesis C of probability C/H . We then obtain evidence, E , which is in favour of C so that C/HE has a much larger probability than C/H , but this result is always considered to increase the value of H that is to say $H/CE > H/C$. If on the other hand the evidence discredited C so that $C/E = 0$ this would be considered as to some extent discrediting H also. If C was implied by H then it would follow that if C was false H was false. But in such a case C is always considered as merely one of an indefinite number of alternatives of which only one can be true. That is to say $(C_1 \text{ or } C_2 \text{ or } C_3 \dots C_n)/H = 1$ and $C_1/C_2 = 0$, $C_2/C_3 = 0$, etc. Therefore to prove one or other of the alternatives false does not necessarily affect our estimate of the probability of H .

We have now two different cases to consider, either the number of alternative generalizations C_1 , C_2 , etc., may be finite or it may be indefinitely large, by indefinitely large, I mean not necessarily infinite but not known to be finite. In the first case we may by eliminating one alternative after another leave a single one master of the field. This in itself does not prove this alternative, (C_r) , to be true; it does indeed show $C_r/H = 1$, but we have not assumed that H is certain only that it is probable. Therefore C_r still requires to be confirmed by obtaining evidence in its favour. Clearly in this case any evidence in support of C_r may support H also and does support it in so far as C_r could not have been implied by any other assumption than

H. While any evidence against C, is equally evidence against H.

This, the simplest possible case, does not often occur, it is best illustrated perhaps by the method of establishing the chemical formulae of an organic compound. In this case our prior assumptions H are the validity of the methods of chemical analysis and of determining molecular weights, the theory of the valency of Carbon, Hydrogen, Oxygen and any other elements concerned, the constitution of certain organic compounds as already established by similar methods as those to be used in the particular case in question, and possibly some other special theory such as the structure of Benzene or the theory of Stereoisomerism. Given all this and assuming the empirical formula and molecular weight to have been determined experimentally, then if the compound is not too complex (a simple sugar, let us say) we can enumerate all the alternative possible constitutions it can have. The majority of these possibilities can be eliminated fairly easily as a rule, and if we are fortunate we shall be left with one possibility only. This is not considered in itself sufficient to establish the formula, for our assumptions are not certain and some of them may be false. Therefore positive evidence of the correctness of the result has to be obtained. The best is the synthesis of the compound. Every successful elucidation of the formula of an organic compound is taken as strengthening the probability of the fundamental assumptions. If there were a compound for which none of the alternative formulae were tenable this would gravely affect our estimate of the value of these assumptions. This instance, from organic chemistry, is not a perfect one because the evidence against any particular formula is not usually absolutely conclusive even if we make all the ordinary assumptions. As a rule all that can be done is to show that some formula is highly probable and others highly improbable.

The other and more complicated case is where we do not know definitely how many alternative generalizations are allowed by our original assumptions. Even if we assume the number of the alternatives to be infinite it does not follow that none of them will have a finite prior probability. They may form a convergent series such that the sum of their probabilities is unity and the first few members of the series have quite a large probability. In any case even if we know the number of alternatives to be large it may not be infinite. The only difficulty is in knowing whether the alternative we happen to have hit on has a finite probability and in comparing the probabilities of different alternatives if we have formulated more than one. We will assume, however, that this can be done. It is clear in this case that the elimination of alternative generalizations by obtaining evidence against them will not in itself greatly affect the estimate we make of the probability of the other alternatives, nor can it adversely affect our estimate of the probability of our prior assumptions (H). On the other hand the confirmation of any one alternative by an examination of the evidence will increase the probability of the assumption in the same way and to the same extent as in the previous case.

What has just been said is intended to be a description of actual procedure rather than a justification of it. Though the procedure seems perfectly reasonable if we consider any particular case. One point that appears to me to be evident from this discussion and that seems to be in accordance with the ordinary common-sense belief of men of science, but might be rather difficult to justify logically is that the probability of the prior assumptions, (H), is more easily increased than diminished by the accumulation of evidence. Evidence in favour of any of the special theories supported by these assumptions is to some extent in their favour, while evidence against any one of the special theories does not necessarily go against them—evidence against C_1 , C_2 , or C_3 is not

necessarily evidence against H , while evidence for any of these is on the whole in favour of H . This of course assumes that the evidence has no direct bearing on H in the absence of the theories C_1 , C_2 , etc., that are under consideration, as may very well be the case if the theories considered do not exhaust the possibilities.

§ 19. Let us now consider the nature of the prior assumptions H which are necessary for any particular scientific investigation. The presuppositions involved in any scientific investigation consist of a set of general propositions about the subject-matter concerned. In the example given above of the establishment of the constitution of an organic compound the presuppositions involved are the ordinary current chemical theory about atoms and molecules and valency and a number of chemical and physical constants by means of which the actual experimental data can be worked up into the form required. All these things are for the purpose of the particular enquiry assumed to be true and provide what a priori probability we have for our generalizations. Now these assumptions are themselves generalizations, the results of previous investigations, and these in turn are based on earlier ones still. Therefore, for any given investigation we use as our prior assumptions a set of propositions H_n , these are derived from a previous set H_{n-1} , and these from H_{n-2} and so on. H_n has been derived from H_{n-1} by means of certain experimental evidence E_{n-1} as H_{n-1} has been derived from H_{n-2} with the help of E_{n-2} . We suppose that each forward step is made with a high degree of probability, and it is to be hoped for the credit of scientific knowledge that these probabilities are very nearly unity, for the probability of the last member of the series is the product of a series of probabilities.

If our procedure is to be legitimate the series must have a first member H_0 so that the whole history of any one department of science may be written $C_0/H_0E_0\equiv H_1$, $C_1/H_1E_1\equiv H_2$, $C_2/H_2E_2\equiv H_3$, . . . $C_{n-1}/H_{n-1}E_{n-1}\equiv H_n$,

assuming that we have no false starts to record but merely smooth progress. The vital point of the whole matter is the nature of our first assumption H_0 and its probability because it is ex hypothesis not derived from any previous term in the way the others are derived from it.

In the first place we must be perfectly clear that H_0 is not going to be the Principle of Uniformity or anything like it. It is not even going to be the principles mentioned by Keynes, important though they are, for their place is really among the later terms of the series.

In the second place we probably ought not to separate H_0 and E_0 but treat them as a unity. H_0E_0 , if we take them together, stands for a large collection of propositions of very small generality. It may be taken to stand for a vague and primitive apprehension of the whole of that region of the external world with which we are concerned. That is to say the total grounds for the conclusions of scientific investigation is to be found in the character of the objects of primitive sense experience and of all that can be rationally constructed from it. Each particular region of investigation has its own region of experience to explore and the validity of its methods and results is only slightly affected by what is found in other regions. In order to justify any special conclusions we need not necessarily appeal to the whole of experience but only to the relevant part of experience. The character of the whole of nature need not be introduced, but only the character of special parts.

There is, it must be admitted, a very considerable amount of interdigitation of investigation that becomes more marked as knowledge increases; so that this separation of 'regions' is not by any means complete. In the early stages of scientific discovery the exploration of different regions goes on more or less independently and it is from this early state of affairs that the traditional classification of sciences arises. At the present day the

boundaries are very much broken down and the old distinctions have become merely conventional and often inconvenient.

§ 20. I have supposed that what is assumed *a priori* in an investigation (H) is such as to give a finite though probably small probability to the generalization (C) it is desired to establish by experiment and observation (E). That these prior assumptions (H) are needed will be granted, by this time, I hope; that experimental evidence (E) is necessary goes without saying; the question that may be disputed is exactly when and how the generalization (C) comes in. It may be urged that such a generalization, even as a surmise, is not required at the beginning of an investigation, the object of which is not to plead a case but to discover the facts. In the second place there is the problem of the logical relations between H and C.

It is an attractive notion that in an investigation we should start with no presuppositions about the state of things to be discovered but with perfectly open minds and a single eye to the facts. There is a fine Baconian smack about it. One thinks of Darwin examining the facts for fifteen years (or whatever the period was) before framing his hypothesis. In fact it is all in the sound English tradition. Nobody can have more respect for the English tradition than I have, so that it must not be thought that I have bowed the knee to any continental Baal when I say that this is all nonsense. Darwin must have had some sort of hypothesis or he would not have known what facts to examine. There were millions of facts and he could not attend to them all. To have an open mind is not the same thing as to have a vacant mind. The vacant mind is like the bottomless pit; no amount of facts will ever fill it. What is absolutely necessary is that the investigator should not allow any hypothesis to give him a bias against the facts. Apart from this the more hypotheses he has the better. I expect Darwin in his account of

his work was thinking of Newton's little joke, "Hypotheses non fingo."

Now we have cleared this fallacy out of the way we can get on. Suppose we have some simple physical constant to determine, the boiling point of an unknown liquid. To begin with we do not know whether the liquid is a mixture or a simple substance, whether it will boil unchanged or will decompose. However, we must have some idea about it or we shall not know how to set about the experiment. If the material is liquid at ordinary temperatures we know it is not likely to boil below 25° C. or above 350° C. We assume, therefore, we can distil it in ordinary apparatus and use an ordinary mercury thermometer. Possibly a chemical examination of the material has provided some information as to where between these limits the boiling point lies. There is the possibility that the liquid will decompose with heat, so that it will be necessary to see if the distillate has substantially the same properties as the original liquid. Lastly the liquid may be a pure substance or a constant boiling mixture, in which case it will all boil over at one temperature, or it may be a single substance (or constant boiling mixture) containing appreciable amounts of impurities, in which case though most of it will boil over at one temperature there will be an appreciable range of temperature between the beginning and end of the distillation, or it may be a mixture, in which case it will boil over a fairly wide range of temperature. Now, although we do not know what the boiling point of the substance is, our general information about liquids and their boiling points enables us to make a list of alternative possibilities as to what the result will be. These alternatives are, to all intents, exhaustive and their logical sum has a very high degree of probability. Our procedure so far would only be illegitimate if we allowed previous assumptions to blind us to any of the facts as they came to be observed. It is to be noted that we must have some preliminary.

assumption as to whereabouts the boiling point will be simply in order to enable us to choose a thermometer, but our assumption is very vague at first. If we want to be on the safe side we will start by using a thermometer reading up to 350° C. for a first rough observation and then will be able to choose a more accurate one with a narrower range for our final experiments.

In this example there is no difficulty in seeing what generalizations can be made prior to the experiment and they are relatively definite and are exhaustive. In very many cases the prior generalizations cannot be known to be exhaustive (except in so far as any proposition and its contradictory is exhaustive) and they may at first be extremely vague, so vague that it is very difficult even for the investigator himself to know they are there or say what they are. As observation progresses they will gradually become more precise and more explicit.

The vital, if not the only, difference between the successful and unsuccessful investigator lies in the capacity to frame the appropriate generalizations that are presupposed in any research. Probably it is more a matter of instinct than of argument. The generalization that is needed may be one that only has a very minute degree of probability *a priori* as far as any logical analysis can show. One may try to avoid the difficulty by saying that it is found by analogy, but that does not really mean anything. Analogy comes in undoubtedly, but in this process analogy is a very slippery guide. There will be a thousand false analogies for one correct one and no obvious means of choosing among them.

§ 21. As has been pointed out by Keynes and others, the mere multiplication of instances does not always lead to an induction having any very large probability, whereas very often we have very great confidence in a generalization depending upon a few instances only. The difference between the two cases lies in the analogy.

If the analogy is weak or unexamined our only method is by repetition of instances and even that does not do very much. If the analogy is strong a few instances suffice. The results of Keynes' analysis of analogy have already been mentioned. The analogy between two instances may be improved, to put the matter briefly, by diminishing the accidental or irrelevant resemblances and increasing the differences. The more nearly vague likeness can be reduced to identity in one respect and difference in all others, the more precise or perfect is the analogy and the stronger the induction founded upon it. There is an incurable vagueness in the character of the objects of every day life which makes analogies between them a precarious basis for generalization.¹ But there is a special device for getting over this difficulty, and that is measurement. By means of measurement we confine our analogy very strictly to one character only and render it as precise as is possible. As the result of measurements, numerical laws such as form the foundation of Physics can be established. These laws are recognized to have the highest degree of probability of any known to us and are capable of being based upon a very small number of observations, provided they are properly carried out. Without measurement we are confined to the use of the rather nebulous notions of cause and effect. By means of measurement and numerical laws we can develop the ideas of functional relations which are eminently suited to mathematical treatment. The next subject to discuss then is that of measurement.

¹ There is a valuable discussion of vagueness and its opposites in Russell's *Analysis of Mind*, pp. 180 and 255.

CHAPTER V

Measurement

§ 22. THE given whole that is presented in experience can be analysed into elements that are similar and others that are diverse. For the present it does not matter "what" it is that is similar or diverse, it is sufficient to assume that it is something that is experienced. When a clock strikes, the individual strokes are similar in so far as they are the same sound, and diverse in so far as each is later than the last. When the clock chimes the quarters the sounds are similar in so far as they are all sounds and not colours or tastes, but different in so far as their pitch is different. It is sometimes argued that we only perceive things to be similar or like one another and never identical, for they always differ in some respect. This contention is false in one respect, because if we only can carry our analysis far enough things that appear as only vaguely like can be resolved into elements that are different and elements that are exactly identical. It is purely a question of how far we carry out the analysis. But as in practice the extent to which it is possible or convenient to carry out the analysis is limited, we are as a rule content with something short of absolute identity. When, therefore, we speak of identity, as a rule we do not mean to say that the two things referred to are absolutely identical, but that they are like each other in certain respects which are important for our particular purpose and that the respects in which they differ are slight or irrelevant. If I say that one orange is identical with another I only mean that they are like each other in certain important respects and that the particular kind of likeness involved

is not analysed any further. When I say that an orange is one, I do not mean that it is impossible to cut it into two, but that for certain purposes I can treat it as one. The fact that for certain other purposes I may treat it as a complex of parts, and so as many, is not relevant.

It does, however, seem to be a fact that diversity is immediately apprehended and identity is only found on reflection and analysis, starting from vaguely apprehended likeness. Certainly our judgments of diversity are usually made with far greater confidence than those of identity. In the process of matching colours, as in the Wool Test for colour blindness, one is certain that some colours are different, but inclined to be dubious as to any two being identical. It is always assumed that there might be some difference if only it could be observed. If two musical notes, say a quarter tone apart, are indistinguishable to my ear, I have no difficulty in believing that they really are different, if anybody asserts that he can distinguish them. But no amount of argument or of assertion of identity by other people could persuade me that two notes I distinguished were really the same. From the theory of sound it is inferred that notes indistinguishable by any other means can be distinguished by the method of beats, and I am quite ready to believe the theory and consider that any two notes that give beats are different even if they sound alike. But if the theory implied that notes I could distinguish were alike, I should conclude the theory was false. This matter has been mentioned already, but it is important enough to bear repetition.

The assertion of identity is, therefore, always liable to contradiction. It is always at the mercy of anybody who can devise some means of increasing the powers of discrimination we possess. But as judgments of identity are in general more valuable and interesting than those of diversity, it is important to find a way out of this difficulty. The method is to treat things known to differ to a small extent as though they were identical.

The device is used unconsciously at the level of common-sense and because unrecognized is liable to lead to grave error. In its scientific use it is used explicitly and the danger of error should be nil. Whether the process is legitimate or not depends solely on the way it is used, and the particular end in view. There is no general *a priori* method of deciding when differences can be neglected and when they must be taken into account. Each case must be treated as a separate problem and a decision made *ad hoc*. As long as no error is observed to result the procedure is legitimate, that is to say as long as no important proposition turns out to have been asserted falsely as the result of what is diverse being treated as identical. It is necessary to emphasize the word "important" here so as to show that purely practical and non-logical considerations come in. The question must be decided in terms of the particular end in view in making the argument.

If it be argued that the end in view is simply the attainment of the truth and nothing more or less; the correct reply is that if it was just the truth and the whole truth we were after it would be impossible to neglect anything because that involves falsification. What we are really after is some particular part of the truth about some particular thing for some particular purpose (the purpose will usually, of course, be to learn as much with as little trouble as possible). Perhaps a little parable will help to elucidate the argument.

"Once long ago (the precise date escapes my memory) a new Librarian was appointed to a certain Library. One morning it occurred to him to examine the catalogue. After some study he turned to an assistant who chanced to be present, and said "I do not think I like this book very much. It is a fallacious and, in a sense, inaccurate document, and what information it contains is trivial. I see nothing said about any book except the author's name, the title, the size of the book, and trifles of that sort. Everything mentioned in here

is to be found in the book itself, and much more beside that is of greater interest. What is put down is correct enough, I suppose, as far as it goes; but to my mind 'suppressio veri' is just as misleading as 'suggestio falsi'; worse, in fact, because the deception is subtler. Suppose now, we abolished the catalogue—" "Oh, sir", interrupted the assistant, "the catalogue is indispensable—it gives the class-marks of the books." "So I have observed" replied the Librarian with scorn; "and they likewise are to be found on the books." "But how", wailed the assistant, "but how are readers to find the books?" "As they do now", the Librarian said, fixing the assistant with a look, as of the stony basilisk, "by looking on the shelves!" The assistant fainted. The Librarian, who was really quite a kindly man, had him taken out into the fresh air where he slowly recovered. In the meanwhile the catalogue was burnt. All these events occurred a long time ago, as I have mentioned."

The facts of the external world stand to our scientific knowledge, or to our commonsense knowledge (there is no difference in principle between them) much in the same relation as a library of books stand to the catalogue. The catalogue does not tell you much about the books but it tells you certain things you particularly want to know, and the fact that the information goes no further does not matter. In the same way our systematized knowledge of the external world misses out a great deal that is really there, but it tells us certain things we particularly want to know. If we really tried to ascertain the whole truth about nature we should never acquire any knowledge at all, because there is much too much to grasp all at once. Therefore it is that we start by neglecting the things we are not immediately concerned with. In any particular investigation we have a perfectly limited end in view, and we endeavour to ascertain just as much as is necessary about the facts involved and no more. The same facts when examined

for a different purpose may require to be treated quite differently, but that does not make any difference. It is really just as bad technique to make a measurement more accurately than is necessary as it is to make it not accurately enough. The good mechanic knows what parts of the machine can be gauged by eye and what parts have to fit to a thousandth of an inch.

To return to the immediate question of diversity and identity. If the question we asked as to whether any particular diversity is negligible or one that requires to be taken into account, the answer must be that it depends upon what the enquirer wants to know. There are no general methodological rules, and each case must be judged on its own merits, just as in matters of moral conduct. The particular type of cases where the calculus of errors can be applied I shall have to discuss later, but these are only one class, and even in these cases considerations of the other sort must be used before any mathematical rules can be applied.

It might be suggested that by insisting on the necessity of the use of considerations that are non-logical and of the importance of the end in view in investigation I am guilty of a breach of "Ethical Neutrality" or at least, to mix the metaphor slightly, am pushing in the thin end of the wedge. The principle advocated by Russell as Ethical Neutrality simply means that we have no business to say a proposition is true (or false) because we think it would be nice for it to be true (or false), without any further examination. It has, indeed, frequently happened that scientific investigators have done this, that they have overlooked differences they did not want to see, or, in vulgar parlance, have cooked their results. But I have no wish to defend this practice, and it is one which leads to disaster in the long run. It is just because no universal rule can be laid down that opportunities for cooking occur. It is always recognized that an investigator is not compelled to give equal weight to all his

observations, but has a right to select those he considers best. If he does his selection properly he remains perfectly neutral ethically. Unfortunately the selection must be to a large extent instinctive. It is the investigator who has the right instinct who makes the epoch-making discoveries and the one who has not got it who cooks his results and fails to make the discoveries. And it is important that wherever rules, such as those provided by the Theory of Probability, can be applied, they should be applied.

Another objection that may be raised is that the Law of Parcimony or Occam's Razor is an infallible rule that is applicable to all these cases, and one that has a general philosophical sanction. Occam's Razor, however, cannot, so far as I can see, be considered as a law of nature. Some of the older physicists used to consider it as axiomatic that "Nature is always simple". Unfortunately the evidence is all the other way, that Nature is very complicated. In fact, Fresnel seems much nearer the mark in saying "La Nature ne se soucie pas des difficultés analytiques." Really, nobody has any means of judging one way or another. It is safest to say that all propositions about the whole of nature are false.¹ Occam's Razor, then, must be taken as being at most a methodological rule and not as a law having any special philosophical prestige. Considered as such, even, it is rather an unhandy instrument and reminiscent of the kind of razor that cuts more off your chin than just your beard. In its most frequently quoted form it asserts that entities are not to be multiplied more than is necessary. But, first, what is an entity; and, second, "necessary" for what? A better way of stating it seems to be "*Frustra fit per plura, quod potest fieri per pauciora.*" This I take as implying that it is a waste of

¹ I need hardly point out that this statement does not involve a vicious circle. What I mean to say is that the assertion of the falsity of all propositions of the n th order about the whole of nature is a proposition of the $n+1$ th order and is true.

effort to carry out analysis further than is necessary to give the required result. This is what has been stated already. But if we are going to treat this rule as one of general validity is it not necessary to be in possession of a second weapon to cut the other way? That is to say analysis must be carried far enough. Perhaps William of Occam also said "*Falso fit per pauciora, quod faciendum est per plura.*"¹ These two rules apart from providing convenient labels do not take us any further than was suggested already.

It is perhaps worth pointing out that the application of any rule such as Occam's Razor to this particular problem, the analysis of experience, is not necessarily connected in fact or in logic with the applicability of a similar rule framed in equivalent words to other problems, e.g. the value of hypotheses. In any case the rule is not a law of logic, but in the proper sense of the words a law of thought. That is to say, it professes to be a maxim for correct or at least successful reasoning on certain problems, but can be neglected without contradiction if desired and does not by itself provide any guarantee as to the correctness of the conclusions. The value of rules of this sort is purely relative, relative to particular minds investigating particular problems. An omniscient Being would not require any rule of this sort, though presumably the laws of logic would be necessary if he was interested in the same aspects of the world that interest us.

Some further light on the character of perceptual judgments may be got by considering a particular case in detail. A good example, suggested by Jevons, is as follows: If I find that the middle C of the organ at York Minster is in unison with a particular tuning-fork and subsequently that the middle C of the Westminster Abbey organ is also in unison with it, I then infer that

¹ At any rate Kant said "*Entium varietates non temere sunt minuenda*", as pointed out by Professor Laird (*A Study in Realism*, p. 124). This saying provides very nearly the maxim required.

the two organs are tuned in unison. We may subdivide the assertions involved.

(1) The pitch of the tuning-fork is observed to be the same as that of a particular organ pipe at York Minster (the middle C) at some particular time.

(2) A corresponding observation is made at Westminster at some other time.

(3) The pitch of the tuning-fork used is constant, *i.e.* is not altered appreciably by the particular time and place of observation.

(4) Therefore it is concluded that the middle C's at York and Westminster are the same.

(5) Etc., given (4) the organs as a whole are inferred to be in unison, as the result of a series of observations on the various pipes of each organ.

Confining our attention to propositions (1) to (4) it is seen that (1) and (2) are both particular perceptual judgments, while (3) and (4) are of an entirely different and more complex type. As regards (1) and (2), it is perfectly obvious in the first place that the sound of the tuning-fork is different in many respects to that of the organ. It is only by analysis of the total experience that it is possible to pick out certain definite elements, intensity, pitch, timbre and so on, and to single out the pitch and decide that in that particular respect the sound of the tuning-fork is indistinguishable from that of the organ. The first time an analysis of experience of this sort is attempted it seems to be very difficult. The two things experienced are felt vaguely to be different and the abstraction of the element of identity is very hard. But once it has been done it becomes easier by practice until finally the identical element is attended to to the exclusion of everything else, and we wonder how ever it was possible to avoid noticing it.

In the case of the pitch of a note it has to be remembered that different observers have very different

faculties of discrimination. When, therefore, I say that the pitch of two notes is identical, all I mean is that I can distinguish no difference and hence that they differ by less than a certain amount, which can be assigned if necessary. Here again it is worth noticing that the fineness of discrimination of any individual can be increased by practice, but it is never legitimate to say that two notes are exactly alike.

These propositions about the pitch of the tuning-fork and the organ pipe are asserted as perceptual judgments and are quite private to the individual asserting them. On the other hand they are perfectly certain and free from doubt, granted that analysis of experience of this sort is a legitimate process.

Proposition (3) is more difficult to deal with. (1) and (2) may be considered to mean approximately the same thing for anybody asserting them, but this is not the case with the assertion that the note of a tuning-fork is constant. If the assertion is made by a physicist it is probably a deduction from the theory of sound and certain mechanical theories he believes in, and may be made quite independently of any particular experience of his own with regard to the actual tuning-fork in question. On the other hand the assertion may be of the nature of a straightforward empirical generalization. In the simplest possible case it will imply that the tuning-fork in question has been observed over a considerable period of time and under varying conditions and its note has always been found to be the same. This does not really mean that the note of the tuning-fork always sounds the same, because that proposition is false. What it means is that any changes in the note it gives are treated as apparent only and not as real changes. The tuning-fork sounds differently according as the hearer is tired or rested, drunk or sober, but these differences are attributed to his tiredness or otherwise and his drunkenness or sobriety. It will usually be said that the sound seems the same whenever it is heard

by anybody who is in a normal condition. Except for the ambiguity underlying the word 'normal' this is a fairly satisfactory statement, but it is difficult to see what its justification is. If we make any distinction of this sort between differences that are apparent only and due to the percipient and differences that are real, it is clear that some general theory as to the nature of the external world is implied, and in particular some view as to the causes of our perceptions. It would make an intolerably long digression to discuss this matter at all fully and in any case I am not at all sure that I could make it clear. The matter may be dismissed more or less summarily by saying that the distinction is drawn partly on empirical grounds and partly as a matter of methodological convenience. We find by experience what sort of changes the percipient as such undergoes and what sort of changes the objects as such undergo. On methodological grounds we never attribute changes to any object wantonly, but only so far as we are compelled to in order to avoid contradiction. For reasons that are not very clear a process of change is *ipso facto* considered as presenting a problem, the permanence of anything unchanged is not; consequently we avoid postulating changes as thereby we should only increase the number of problems.

To take the case of the tuning-fork, besides emitting a characteristic noise when struck it has also a certain size, shape, colour, weight, hardness and so on. There is a considerable constancy of all its characters that are considered important and we are loth to believe that its note changes unless the evidence for the change is extremely cogent. Our mere recollection of sounds we know by past experience is not a sure guide as to sameness or otherwise, it is in fact very fallible. We may, therefore, assume at least provisionally that the note of the tuning-fork is constant; further experience will show if we are right or wrong, and in the meanwhile this is the simplest assumption to make.

The argument about the tuning-fork may be concluded briefly by saying that propositions (1) and (2) are perceptual judgments of identity, proposition (3) is an assertion that the tuning-fork is taken as a physical standard. From (1), (2) and (3) then we can legitimately infer (4) that the two middle C pipes on the organs are the same. From this by additional arguments all of the same type and which there is no need to discuss we conclude that the two organs are in unison generally.

This example provides a very typical case of the devices whereby if two things are difficult to correlate some third thing is introduced with which each in turn can be correlated. No one would attempt to correlate the organs at York and Westminster by listening to the one and then catching the express at King's Cross and going and listening to the other. The tuning-fork is introduced as a standard with which either can be compared at will. There is no a priori guarantee that the standard we choose shall be really constant. We never wantonly assume that a standard must be constant, but on the other hand we never wantonly assume that it will vary ; it is only when discrepancies in our judgments definitely lead us to look for variations that we think about them. The problem of what to do when a standard is found to vary is not merely academic but one of great practical importance ; for in this imperfect world invariable standards are not easy to find. One method adopted is to correct the changes in the variable standard by means of something assumed not to vary. For instance, the standard of time, the period of the Earth's rotation, is corrected on the assumption that the Newtonian laws of mechanics hold exactly. In most cases, however, it is possible to correlate the changes in the standard with some other observed change and to make the correction in this way ; for instance changes in standards of length are correlated with temperature changes and so corrected for. It must be noted that

provided the corrections can be made with the required degree of accuracy there is no theoretical objection to using a variable standard which needs correction. In physics correction does not have any ethical significance, as it has in the case of small boys.

An interesting point that arises out of the fallibility of the senses and particularly of sensory memory is that endeavours are always made to simplify the actual perceptual acts in observation as far as possible. To this end the most elaborate mechanical devices are introduced. Observations are reduced in the highly developed sciences to such acts as judging spatial contiguity, betweenness or coincidence or the threshold of recognizable diversity of colours, sounds, etc. In the less developed sciences where observations are necessarily more complex they are recognized as being much less certain and precise.

§ 23. For the process of measurement something in addition is needed besides an act of comparison, such as is described in the previous section. The comparison results in a judgment of identity or diversity, but we want to be able to make a further judgment by which a numerical value is assigned to the object which is being measured. The purpose of this is well expressed by Dr Nunn,¹ who says "Since primary facts present themselves for the most part in series, the most useful method of determining the objective consists in correlating the terms of these series with the number series—the property of this series being that single members of it can be substituted for combinations of other members in accordance with definite laws easily applied," *i.e.*, the laws of arithmetic. That is the purpose of measurement, but the nature of the process requires further analysis. The houses in a street form a series and we assign numbers to them, yet we do not call this measurement, because the properties of their numbers give us no information about the properties of

¹ *Aims and Achievements of Scientific Method*, p 143

the houses. What we know of Nos. 2 and 3 does not tell us anything about Nos. 5 and 6, although $2+3=5$ and $2 \times 3=6$. If the houses had been designated by the series of square numbers, the letters of the alphabet, or the names of the Kings of Israel in chronological order, it would have done just as well. In this process of numbering, numerals are assigned simply because they have a serial order which can symbolize the order of the houses, not because they are a series having special logical properties. Hence it is necessary to distinguish a process like numbering houses in a street which involves only the ordinal properties of numbers from measurement proper.

Measurement must also be distinguished from counting. Counting is a necessary part of measurement but is not itself measurement. Briefly, in counting we assign cardinal numbers to groups of things, in measurement we assign a ratio to represent some property of a thing. The numbers we write down to stand for the results of measurement are always ratios not cardinal numbers. When we say that a thing weighs two pounds we mean that the ratio of its weight to the pound weight is two. When we say a man has two legs we are making a different type of assertion, namely one about a cardinal number. This is seen from the fact that we do not mention a unit, and that we are confined to one definite number. As long as we are talking of legs any man has two of them and nothing except cutting one off can alter that. The series of cardinal numbers is discontinuous and each corresponds uniquely to one set of aggregates of things and no other, i.e. all the things that have that number. Thus the number two is simply all the pairs in the world, including men's legs. But when we are stating the weight of a thing, according to the unit of weight chosen, we can make any numeral stand for its weight. If it is pounds we are using, its weight will be one thing, if grams, another.

Further, the laws of addition of measured values are

those of addition of ratios not of numbers. That is to say if $\frac{x}{a}$ and $\frac{y}{b}$ are two measured values $\frac{x}{a} + \frac{y}{b} = \frac{bx+ay}{ab}$.

As a matter of convenience we always choose our units so that $a = b = 1$ and the expression simplifies to $\frac{x}{1} + \frac{y}{1} =$

$\frac{x+y}{1}$.¹ When we speak of addition in connection with measurement we always mean some physical manipulation of things which in respect of the particular property considered is equivalent to the addition of the numerical values. For instance, if we want to add two lengths together we do not lay two rods of the required length alongside each other but end to end, in a straight line. The character of the manipulation depends on the property to be measured and the laws holding among the bodies manipulated. If I have a gas of a given density I can add two portions of it to give a gas of twice the density, by compressing it to half its volume. But I cannot do this with a liquid. This is not because of anything inherent in the property of density, but simply because I have no means of applying sufficient pressure to a liquid to carry out the manipulation. It so happens that density is a specific property of liquids and is only variable within very narrow limits, while it is not a specific property of gases (in the sense of being almost independent of pressure and temperature) and is variable within extremely wide limits. However, even in the case of liquids I can ideally describe an experiment which would enable me to add densities even though I cannot actually do it. The fact that I know how to set about varying the density of a body in numerical ratios shows that it is a measurable property. There are properties which cannot be varied in that way either actually or ideally and these are not measurable in any proper sense, although numerical values may be

¹ We could, if we wanted, make all our calculations in terms of the reciprocals.

assigned in virtue of a process of comparison. The property of hardness is not additive and is not measurable. What is called measurement of hardness consists in setting up a series of standard bodies and arbitrarily assigning numbers to them, as to the houses in a street, and then seeing which of them scratch or are scratched by the body whose hardness we have to "measure". But the number obtained by this process and made to represent hardness is entirely arbitrary and is not really a measure of hardness at all. We do not know enough about the laws governing the hardness of bodies to manipulate them (even ideally) so as to add or subtract hardness, as we can densities.

The distinction then between measurement proper, such as that of lengths, weights, densities and so on, and quasi-measurements such as that of hardness lies in just this, that in the former case we know enough about the laws of the properties to manipulate bodies so as to add or subtract the property in question. Sometimes the manipulation is difficult or it may even be impossible. But in the latter case we at least know how it ought to be done even if we cannot do it. On the other hand where we know nothing of the laws of addition of the property we have to be content with quasi-measurement. In the case of quasi-measurements there are degrees of approximation to true measurement according to the extent of our success in manipulation. In the case of hardness we have practically no power of manipulation (and the ordinary scale does not even always give consistent results). The measurement of temperature, before the thermo-dynamic scale was devised, is an example of a more satisfactory but still not perfect approach to measurement. It approached measurement because we can vary the temperatures of bodies in a regular way, but was imperfect because the definition of equal increments of heat was arbitrary, depending on the properties of an arbitrarily chosen thermometric substance, mercury. Thermo-dynamic theory enabled

the arbitrary element to be removed and made temperature quite strictly measurable.

As measurement must be distinguished from quasi-measurement on the one hand, it must be distinguished from the process of counting on the other, as has been mentioned. All material bodies as far as they can be discriminated as distinct in space and time and occupy any appreciable extent of space and time are numerable, provided they can be identified and recognized. We can count the drops of water poured into a glass if the water goes in drop by drop, but once in the glass we cannot count them because we cannot recognize them. One further point is that things that are numerable must have some qualitative resemblance. If we say of any things that they are two, we must be able to answer the question "Two what?" That is to say whatever things are numerable must be spatially or temporally discrete but qualitatively similar in some respect. It is this that makes counting the basis of measurement.

Of course there are numerable things that have no spatio-temporal relations. We can say that colours have three different properties, hue, purity and luminosity, or that there are seven deadly sins, and so on. Such cases need not concern us greatly. Suffice to say that entities which are not physical may be numerable if they are discrete in some respect and similar in some respect.

The chief points of contrast between counting and measurement are (1) the number of things counted is unambiguously fixed, as already explained (2) the process of counting and of addition of counted objects does not involve any physical manipulation of them because their number is independent of their actual position in space and time. In order to add the population of Scotland to that of England it is not necessary to make anybody cross the border. Physical manipulation may be an aid to addition, but it is no integral part of the process.

The measurements used in scientific practice, as in everyday life, are either measurements of the geometrical properties of bodies or are derived from them. This derivation of other metrical properties from geometrical ones is due both to practical convenience and to theory. For mechanical theory it is the extension of bodies in space and time that is their ultimate property from which other properties are derived. Therefore if we can measure with sufficient accuracy and detail the relative spatial and temporal configuration of particles we shall have attained as complete knowledge of them as possible from the point of view of theoretical mechanics. Hence our theoretically ultimate measurements must be of distances, angles and time intervals—what may be conveniently lumped together under the name 'geometrical properties and relations' treating time as an extra dimension.

In this matter practice is actually in advance of theory. Mechanical theory has not yet got as far as to describe every property of things in geometrical terms or even every metrical property, but in practical measurement whatever the kind of property measured, what is actually observed is a geometrical property or relation, usually a length, sometimes an angle or a segment of a circle. There are comparatively few measurements that do not resolve themselves into reading off a length on a scale. Even time measurements, though theoretically they must have an independent derivation, are carried out by this means in practice.

Is it possible to develop a system of measurement independent of the geometrical properties and relations of bodies? The answer is, I think, that is possible, though not useful or convenient. In measuring a weight on a balance in the ordinary way we are applying the laws of the lever and of gravitation. We are treating weight as a derived property obtained by means of a knowledge of mechanical laws from observations of geometrical relations of bodies. I don't mean to say

that nobody ever used a balance until the laws of the lever and so on had been worked out, but that these laws supply the rationale of the process. If therefore we wish to obtain measurements of weight directly and apart from mechanical theory we must discard such mechanical aids as balances. This it is possible to do. By the process of lifting bodies in the hand it is possible to judge directly of their equality or inequality of weight, at least roughly. If we are content with comparatively crude results and with a restricted range of weights we can weigh bodies perfectly well in this way without recourse to any theory by a direct appeal to the senses. The results so obtained will have all the characteristics of weights obtained in the usual way except that of accuracy; in particular we can assign numerical values to weights and add and subtract weights and carry out all ordinary manipulations. The only reason why such a system is not developed is its lack of exactitude. The most exact direct judgments of comparison we can make are those of length, consequently any measurement we wish to make with accuracy we convert into an observation of length by suitable mechanical devices. And we do this even if we have other sense organs that could do the work. We cannot construct and use instruments of measurement for making these conversions without a knowledge of the mechanical laws of the working of the instrument. It is acknowledged again that balances are older than mechanics, but prior to some development of mechanical theory no great refinement or certitude was possible. Moreover, the law of the lever, which is the fundamental one for the theory of the balance has been known in a vague sort of way from time immemorial, and it is only in so far as people had some apprehension of its character that their use of balances was rational.

Supposing a system of direct weight measurement without balances and independent of mechanical theory had been developed, it would ultimately be necessary

to correlate weight measurements with geometrical and mechanical knowledge independently obtained. Then it would be seen that from the mechanical point of view weight must be treated as a derived and complex magnitude and must be related on the one hand to volume—a purely geometrical property—and on the other to mass, a property that is not measured directly. It would also appear at once that the direct measurement of weight was very inaccurate, and it would be superseded by the indirect measurement by means of the balance. Weighing with a balance means, of course, watching the movement of a pointer over a scale, counting the weights on a pan and seeing the position of a rider on the beam—all these are purely geometrical observations.

Probably many properties besides weight could be measured directly and independently of the geometrical properties, but it would not be convenient to do so in their case either. Both theoretically and practically then the fundamental metrical properties and relations of bodies are spatial and temporal. In particular the fundamental units of measurement are length and time and all others are constructed from them, with the addition of such other properties as are found to be necessary, such as *mass*.

The process of measurement as has been mentioned always involves some manipulation of bodies; it is experimental in a sense that counting is not. The process consists in the comparison of two things in respect of some property. One of them is taken as the standard in terms of which the magnitude of the other is expressed. If the standard used and the object to be measured happen to be equal then the comparison is a simple affair, but as they usually are not it is apt to be somewhat complicated. The most convenient procedure usually is to take a standard that is small relative to the magnitude to be measured or to subdivide the standard, and to find by repeated application

in the correct manner to the unknown how many times greater the unknown is. Or, of course, if we have a small unknown it can be applied repeatedly to the standard. There are two characteristics of the process that should be mentioned. (1) The actual numerical value assigned is not fixed except by fixing the unit of the standard, (2) even the fixing of the unit does not fix the value absolutely but it is determined as lying between certain limits. That is to say the error of measurement is something inherent in the process; though it can always be made smaller than any given amount it can never be eliminated. It is only in theory too that it can be indefinitely reduced; in practice the difficulties increase so rapidly with each diminution in error as to set a fairly definite limit at any given stage in the progress of knowledge.

A further point that must be mentioned is that a theory of congruence is required to form a basis for any system of measurement. That is to say we must have some reason for saying that A is equal to B and not to C, and some rational method of procedure for the repeated application of one magnitude to another for purposes of addition.

§ 24. Let us first of all consider a simple and rough type of measurement such as pacing out a distance on the ground. The standard in this case is the length of a pace, and the measure of the distance the number of paces taken. The distance will not be any exact number of paces, though in some cases the differences will not be noticeable. Generally it will be obvious that the distance is greater than n and less than $n+1$. If the pacing has been done with precision and the conditions are suitable, we shall find however often we carry out the process that the distance is between n and $n+1$. This kind of uncertainty is what I shall call the inherent error of the measurement. Its size is determined by the "step" of the instrument, to use Campbell's convenient term, which in this case is the

human pace. That is to say if a distance of one pace is the least we take account of, the greatest exactitude attainable is to determine the value of the unknown as lying between two consecutive numbers of steps. By modifying the procedure the "step" can be reduced in size and consequently the inherent error. Thus, instead of pacing out the distance we might measure it with a yard stick divided in inches and reduce the step to an inch. Or if we liked we could reduce it to a tenth or hundredth of an inch without any difficulty.

Sometimes the length to be measured will appear to coincide exactly with a definite number of yards or inches or whatever it is that is our unit, but this exact coincidence is an illusion. If more refined methods of comparison are used a difference will appear. In geometry we can deal theoretically with lengths that bear an exact ratio to one another and we can define units that bear an exact relation to one another, but in actual measurement there is a certain vagueness due to the imperfection of our sense organs and other instruments that makes exact comparison impossible. We know also that theoretically in many cases there will be no exact ratio and even where there should be one the chance of hitting on it is infinitesimal. The early Pythagoreans appear to have had what would now be called a quantum theory of space. They considered that lengths bore exact numerical ratios to one another and in accordance with this belief represented lines as rows of dots. But they soon discovered their mistake when they came across incommensurables. That is to say they found if a number was assigned to one length such as the side of a square no number could be assigned to the diagonal, although its length could be bracketed between two ratios which might differ by as little as they liked. This theoretical discovery that there is not necessarily an exact numerical ratio between any two lengths, when it is applied to actual

measurement takes the form that there is never an exact numerical ratio between observed lengths.

If the step of the instrument be reduced sufficiently in any process of measurement another type of error besides the inherent error becomes apparent, which I shall call the fluctuation of measurement. The fluctuation is what is usually discussed in the treatment of errors of measurement, as measurements are most often made so that the inherent error is smaller than the fluctuation and not apparent. If we are measuring a long distance over rough ground instead of finding each time that the distance is between n and $n+1$, we shall get a different value at different trials. This kind of error, the fluctuation, cannot be reduced by altering the type of instrument. For instance by using a tape divided in yards instead of pacing we may be able to reduce the size of the fluctuations, but if the fluctuation is of the order of a few yards we shall not reduce them in the least by using a scale divided in inches and expressing the results in inches.

It should be obvious that with any given type of instrument the best result is attained when the size of the step and the inherent error is of the same order of magnitude as the fluctuation. If either the one or the other is very much larger we are not getting the utmost out of the instrument. In many cases it is much easier to reduce the size of the step of the instrument than to reduce the size of the fluctuations ; because the step can be reduced by a simple magnification of movements by optical or mechanical means ; whereas to reduce the fluctuations necessitates better control of external conditions, and the ferreting out of unknown variables. These considerations are of great practical importance because a singular delusion often persists even among trained scientific observers, that simply by reducing the inherent error of a measurement or in other words increasing the sensitiveness of the instrument, the accuracy of the measurement can always be increased.

The accuracy of the measurement can only be increased by reducing the inherent error when this is largely relative to the fluctuations. Now that electrical technique has developed to such an extent that the results of almost any physical change can be magnified as many million times as the observer likes, this kind of illusion is specially dangerous. Increasing the sensitiveness of the instrument beyond a certain point unless the fluctuations can be diminished to a corresponding extent may simply result in making measurement impossible, because the effect to be observed is swamped by chance variations.

If the result of repeated measurements of the same thing is to get a number of different values, something has to be done. We want one value not a whole host of values. As is well known the way out of the difficulty is to take an average. As is not quite so well known, there are an infinite number of ways of taking an average of a group of numbers. Provided we have a definite rule for finding the average and always use it correctly, one rule is as good as another, for most purposes. The valid reason for taking the arithmetical mean is simply that it is easy to calculate, and that in most cases the results are symmetrically distributed about a median value and consequently every obvious way of taking an average gives almost exactly the same result. But there is nothing sacred about the arithmetical mean. In many cases there is a lot to be said for taking the square root of the mean squares as the average; but this is troublesome to calculate. The careful experimenter does not usually waste his time on complicated arithmetical manipulation of his results, but prefers to spend it improving his apparatus. Therefore, in spite of all theoretical arguments, the arithmetical mean will remain his favourite. It is the inferior observer who tries to make up for bad experiments by fancy calculations.

The discerning critic will say, of course, in reply to this, that I seem to have forgotten that there is such,

a thing as a calculus of errors from which all sorts of valuable results have been obtained. I must ask him to be patient for a moment, while I explain.

It is often supposed that the aim in taking an average of a collection of experimental values is to get an approximation to a supposed true or real result and that there are reasons for supposing that one average may get nearer to this real value than another. Making a measurement is supposed to be analogous to shooting at the bull's eye on a target. If there are no constant errors, *i.e.* if the range is known exactly and the wind gauge is correctly set, the shots will be distributed over the target in a uniform manner about the centre of the bull so that a properly worked out average will coincide exactly with the centre. The case of making a measurement is analogous to shooting at a target, if we assume the target to be an infinite surface and the bull's eye to be visible from the firing point, but invisible to the marker, who sees only a plain white surface on which the shot marks appear. The marker will not know where the bull is, and the man who is firing will see the bull but will not know how near the shots are going. Unless there is known to be a constant error—and that can only be discovered indirectly—the marker will assume with some degree of reason that the bull lies somewhere among the group of bullet marks but any choice he makes as to its exact position will be arbitrary.

The observer who imagines there is some real or true value, which can be attained by correctly calculating an average from a collection of different numerical results, is guilty of the absurdity of thinking that arithmetical calculations can supply better information about the external world than actual observation. The fact is that the true or real value of anything measured is simply all the results of the different attempts to measure it. That is to say given some particular instrument of measurement and certain conditions of observation the result of the measurement is simply the

class of all the values found. The case is similar to those cases that puzzled mathematicians where a series which did not converge to a limit had to be dealt with as though it had a limit, but it is more complicated. In the first place because we only have a small selection of observed values out of the whole class of observable values and secondly because for purposes of calculation we have to assume that the fluctuations are all chance fluctuations and that there are no systematic or constant errors, an assumption that can never be proved and is usually far from correct. However, the ordinary calculus of errors is an attempt to work out the formal properties of such collections of numbers as are obtained in a series of fluctuating observations. The average that is used is simply a symbol that is used for convenience to stand for the whole class and which, as it is obtained by calculation and not arbitrarily, has certain definite formal relations to the known members of the class. Thus, although no one average is more real than another, but all are merely symbols, there will be perfectly legitimate grounds from the point of view of the calculus of errors for preferring one to another to signify the class for certain purposes, according to its special formal relations to the individual members. At the same time for all ordinary purposes the arithmetical mean in virtue of its simplicity is bound to be the most useful.

§ 25. As has been said already the geometrical relations and properties of bodies are the fundamental metrical properties from which all others are derived. In ordinary practice the actual observation is of the length of a straight line, sometimes of an angle or a segment of a circle. Even time measurements are made in this way, though they can be made directly and must for theoretical reasons be treated as primitive. We must therefore deal first of all with geometrical measurements apart from time.

In order to establish a system of geometrical measure-.

ments we require to be able to make certain judgments which should be as far as possible direct primitive perceptual judgments as little contaminated by theory as is possible.

We must in fact be able to say (1) what lines are straight, (2) what lengths are equal, (3) what angles are right, (4) what lines are parallel. (1) and (2) are indispensable; (3) and (4) we do not need if we allow ourselves the privilege of using Euclidean theorems for constructing right angles and parallel straight lines with the help of judgments of straightness and equality only. But as a matter of fact we do, I think, make use of our direct sense of rectangularity and parallelism though our judgments of them are less easy and certain than our judgments of the other properties.

The discerning critic, who was with difficulty kept quiet before, is now aching to know whether I believe Space to be Euclidean or not. But that I shall not tell him, at least not yet. Instead I should advise him to go and ask the people who make the most accurate spatial measurements, namely the astronomers at Greenwich Observatory, and the Officers of H.M. Ordnance Survey, and hear what they say. Then I should advise him to read Whitehead's *Principles of Natural Knowledge*, and *The Concept of Nature* and Part VI of *Principia Mathematica* (which nobody has ever read); and after that, if he has any opinions left, I shall be delighted to hear them.

Measurements of length are usually made by juxtaposition of the unknown with a scale; and all measurements, however they are made actually, are derived from a juxtaposition of scales, because that is how the standard of length is used. As every scale is a subsidiary or relative standard of length we need not bother at present about the reference back to the final standard. A scale fulfils several functions. It is (1) a standard of length, (2) a standard of straightness, (3) a convenient mechanical device for adding together units of lengths in the

correct manner without overlapping and without gaps. Functions (1) and (2) are fulfilled in virtue of its being a rigid body. The doctrine of rigid bodies is a difficult one and will be discussed shortly. In the meantime function (3) needs a little explanation.

Whatever units we care to express our results in, in using a scale the working unit is the smallest scale division to which our comparison extends, complications due to estimating fractions of divisions need not concern us. For instance, if we are using a scale divided in millimetres and measure to the nearest millimetre, the procedure is to find the two lines on the scale to correspond most nearly to the limits of the length to be measured and then to count how many millimetres this is. The function of the scale is to add up millimetres. The actual process of repeatedly adding a length to itself is only possible with fairly long things and is laborious and inaccurate. Of course there are still other ways of making the measurement but they do not involve any new principle.

When a scale is juxtaposed with something to be measured, the judgment that the unknown is equal to a certain number of scale divisions is simple and immediate, but we require further judgments that the scale divisions are all equal, that they remain the same even when not juxtaposed, and that they are the actual length they are supposed to be. These judgments do not appear to be immediate but to involve a belief in the rigidity of the scale; which belief would be false if the scale were made of india-rubber and is never exactly true of any material. The doctrine of rigid bodies is in fact a highly sophisticated one. On the other hand the discovery that bodies are not rigid is due to measurements. But the measurements depend on the use of scales, which must be assumed to be rigid. Apparently it is easy to discover that some bodies are less rigid than others, but it would be necessary to assume some absolutely rigid substance to make a standard metric

body with which others can be compared. Yet this is not done. The standard metre is, of course, made of the most rigid obtainable material, but this is not supposed to be perfect and unalterable; in fact people profess to be able to measure changes in its length. There seems to be a fallacy somewhere. Appeal to the wave-length of the cadmium flame is not any help, because apart from theory depending on measurements with ordinary scales we should have no earthly reason for supposing its wave-length to be constant.

The solution of the difficulty is fairly obvious. We do not depend ultimately on any supposed absolute standards, or on the theory of rigid bodies or on juxtaposition, but on immediate and simple perception of equality—and also of straightness. If you ask an ordinary unsophisticated person whether a rod is straight, he closes one eye and applies the end of the rod to the other and looks down it. He then says it is or it is not, according to his ability. If he is asked whether the scale divisions of a measuring rod are all equal he holds the rod straight in front of him in a good light and looks at them. Perhaps if some look a little different to others he takes a pair of dividers to see whether his suspicions are confirmed. But in the end he depends upon what he sees. He knows of course that if a straight rod is half in water and half out, it will look crooked; but he does not let that worry him, he simply avoids making tests of the straightness of rods under those conditions. That is to say he knows by experience that certain conditions are bad for making accurate judgments and avoids those conditions. The physicist uses much more refined methods and is far more critical, but essentially his methods are the same. He depends ultimately upon what he sees, but he takes care to get the maximum possible degree of discrimination out of his sense organs by the use of any device he can find. He avoids all conditions he has reason to believe give rise to erroneous judgments. Lastly he is careful if possible to make the

comparison by different methods as a way of avoiding unrecognized sources of error. But in the end he is driven to say that things are equal because they appear equal and are straight because they appear straight.

The psychologists and the physiologists have taken a lot of trouble to ferret out all the defects of the human eye, with the result that they grossly slander that admirable instrument. Given favourable conditions our eyes have an extraordinary aptitude for geometrical judgments and are by no means easily deceived. Unaided judgments of straightness or curvature, of normality, of the abutment of bodies, of coincidence in the line of sight are extremely acute. The eye is also very sensitive in judging of equality and inequality of lengths. The accuracy with which a straight line can be bisected by eye is astonishing.

It is to be noted that in all this no allusion has been made to transmission theories of light, the optics of the eye, or the nervous system. Any such allusion is inadmissible and would vitiate the whole argument. Even quite acute thinkers, when discussing the method by which scientific knowledge can be built up from primitive sense experience, seem inclined to drag in all sorts of nonsense about waves in the ether and retinal images and nervous impulses and any scientific fad that happens to be going. All this is theory (usually very bad theory) that is based on scientific knowledge and cannot be used to explain it.

There is one serious difficulty which has not yet been dealt with. Admitted that our judgments of comparison are infallible within any single experience, it still remains to see how we can make comparisons of things remote in space and time without some doctrine such as that of rigid bodies. This matter has been discussed to some extent already, but a few words more are needed here.

Our judgment that the properties of a body have not been altered by change of place or lapse of time, e.g., that a standard of length remains constant, depends

on the absence of any observed change. There does not seem to be any more satisfactory criterion. If continued observation under widely varying conditions does not reveal any change, then we assume that the standard has remained unchanged. If changes in it can be observed with changes of condition these do not matter provided they (1) follow a regular law, (2) are completely reversible, and (3) are reasonably small, so that we can bring the standard to a definite condition. For instance the fact that standards of length vary with temperature does not preclude their use as long as a definite length is reached reversibly corresponding to a definite temperature. It is worth noting that the temperature measurements necessary for the accurate standardization of a measuring rod require measurements of length, but since an extremely large error in these would only introduce a minute error in the standardization no vicious circle is involved.

As has been said, then, we assume a standard to be constant unless we have positive evidence that it varies. Unless this assumption is made, measurement is impossible. The state of affairs is not quite so bad as it sounds. First, because the kind of weakness is merely that common to Induction and all human reasoning, that the unforeseen must be treated as non-existent. In the second place the difficulty of observing variations in a standard is itself a guarantee that variations if they exist shall be small. Any standard that is in use may vary but the variations are not likely to be large enough to affect the majority of measurements. Variations are most likely to be discovered only when unusually exact measurements are being made and if discovered then and controlled will be quite innocuous.

In the treating of measurements of length apart from measurements of time I do not wish to imply that ultimately time and space are disparate. Rather I hold with Whitehead that the ultimate ingredients of nature are events with extension in time and space.

But though time and space have the same root, for the purposes of general knowledge we had best consider them abstractly as the timeless space and spaceless time of classical physics. For the purposes of measurement that is, it is convenient to use the constructs and not the basic material. As a matter of fact time measurements involve certain special problems which have to be treated by themselves.

The critic who was with difficulty silenced over the question of the Euclidean character of space will be getting restless again and will want to know where the Principle of Relativity comes in. The answer is that it does not come in; we are concerned with important topics and not with trivialities. Relativity is based on certain very special and very minute discrepancies in the results of measurements made in accordance with the classical theory, but all the data on which the Relativity Theory is based and the verification of its conclusions depend on measurement. Therefore, measurement is logically prior to this theory and it cannot be appealed to in criticism of measurement, except in those very special and remote cases where discrepancies arise and there it is not the measurement that is impugned but the theory originally based on it.

§ 26 There are special peculiarities about the measurement of time intervals which do not occur in any other form of measurement. The experience of lapse of time, as has often been pointed out, is inseparably connected with the motion of bodies, and the difficulties that occur in its measurement are due to the difficulty of ascertaining what motions are uniform and so suitable as time measurers. There is a further difficulty, though a lesser one, due to the fact that different means have to be used for measuring short and long intervals of time, and the two sets of measurements need to be correlated. Long intervals are measured by means of the periodic motions of the heavenly bodies (the rotation of the earth), short intervals are measured by clocks which are essentially

devices for obtaining uniform rotation. As it happens that the errors of time measurements are absolute, not as in the case of most other measurements relative to the total magnitude measured, the long period measurements are more accurate than the short period ones. Hence clocks are corrected by means of astronomical measurement, and the latter supply the working standard though not, as I believe, the absolute standard.

The chief difficulties in time measurement turn on the definitions of equal times and of simultaneity. Equal times are sometimes defined as follows: two periods of time are equal in which two physical operations, of whatever character, take place which are identical in all respects except lapse of time. This mode of definition is thoroughly vicious. It robs us of the possibility of correcting one clock by means of another, because it would, by definition, make all reasonably well constructed clocks true timekeepers. The confusion that would follow the serious acceptance of this definition would be indescribable; not a single law of nature would remain true. The fact is, if events are apparently identical we have reasonable grounds for expecting the times to be identical too, but nothing more. If the times are not identical after all we expect to find other differences, but must have independent grounds for judging of the equality of the times.

Poincaré boldly accepts the paradox and says that time measurements are purely arbitrary.¹ His contention is simply that that system of time measurement is adopted which allows the Newtonian laws of mechanics to be true, and the justification of the procedure is that any other system would make the laws of nature too complicated. He argues that there is no reason to consider the intervals between successive noondays as equal, rather than the intervals between successive sunrises,

¹ *La Valeur de la Science*, Chap. II. Poincaré's views are criticized by Whitehead (*Concept of Nature*, pp. 121-4), who also maintains the primitiveness of our experience of equality of times (p. 137).

or between sunrise and sunset, or indeed any intervals such as the interval between meals. The reason for regarding the intervals between successive noondays as equal is simply that it makes Newtonian mechanics possible. If we supposed the intervals between meals to be equal there would be no laws of mechanics and the laws of gastronomics would be the fundamental laws of science.

This argument is plausible only as long as we are thinking of fairly large intervals of time, which are too long to make one single experience. An exactly parallel argument about measurement of lengths would be plausible as long as we thought only of lengths of several miles. I believe that in the case of short time intervals we have a primitive perception of equality and inequality just as we have in the case of spatial equality if the spaces are small enough. In the case of time intervals it is hearing which is sensitive and the perception is that of what is commonly called Rhythm. When an event is periodic and the period is short enough for several whole periods to come into a single experience, to come within the "specious present", the ear can easily detect whether the periods are of equal or unequal duration. The sound of a clock ticking, an engine running or a musical instrument playing, all give the right sort of experience. We are extremely sensitive to any irregularity in rhythm such as a petrol engine "missing" or a clock with a bad escapement that changes its time occasionally. And in the case of music we are sensitive even to a gradual acceleration or deceleration of rhythm. One is so accustomed to think of hand and eye as the discriminative sense organs par excellence that it is easy to overlook the powers of the ear in those directions it has specialized in.

Measurements of time that have any pretence to accuracy all depend on some periodic repetition of events, which are such that the periods are immediately perceived to be constant. I take it that the people who

first made waterclocks relied on them because the drip of the water from the funnel could be heard to be regular. It seems very unlikely that they were calibrated consciously or unconsciously with the length of the day until they had become recognized time-keeping instruments, recognized independently. Galileo is said to have first realized that the period of a pendulum is constant and independent of the amplitude. But I have not found a clear account of how he came to this conclusion, apart from the story that he watched a lamp swinging in the Cathedral of Pisa and timed its period against his pulse rate (he was a medical student at the time). Whether apocryphal or not the story is perfectly plausible and Galileo's procedure appears rational. The rhythm of the pulse of a healthy man at rest and not emotionally excited is remarkably constant and is of the sort of period that can be immediately perceived as constant.

The laws of motion are not *a priori* but depend on certain observed facts. The facts required are comparatively few in number. What are necessary are Galileo's law of freely falling bodies, the laws of the oscillation of the pendulum, and the laws of impact and so on. The discovery of all these laws involves time measurements. Unless the system of measurement used is logically independent of the laws of motion the whole science of mechanics is a gigantic fraud. If Galileo said to himself, "I will assume that the wag of the pendulum measures equal times because by so doing I shall arrive at simple mechanical laws" he deserved to be burnt by the Inquisition, because instead of starting an experimental investigation of the external world, he was merely asserting a number of identities of definition and his experiments were only "eye wash." Myself I think better of Galileo. I believe that he really considered his time-keeping instruments to be true time-keepers, independent of any theory and that he based his belief on primitive experience.

In his experiments on falling bodies he used a water-clock of his own devising. The instrument consisted of a large vessel of water with a small aperture he could open and close with his finger. The volumes of water that escaped he measured by weighing. As the head of water was practically constant, equal weights measured equal times.¹

Assuming that we have a primitive perception of the equality and inequality of times, we can make clocks which satisfy our perceptual requirements approximately. A study of these clocks will show that the times of the solar day and the solar year are approximately equal. This result is what would naturally be expected. That is to say as the period of the day and the year mark a natural periodicity of human functions and of nature generally, our natural prejudice would be in favour of considering them equal times, though we might also vaguely tend to consider the periods of daylight and dark as equal, even in high latitudes, if our activities were completely regulated by them as those of most animals are. However, it so happens that our clocks and any vague primitive ideas we may have about the longer periods of days and years are in agreement, and it is idle to speculate what we would have done if they had been in disagreement. As our clocks are all liable to mechanical defects and require adjustment it is convenient to take the comparatively long periods of days or years to correct them by, and the observations required to do this are very easily carried out with great accuracy. However, observation soon shows that the apparent solar day (as shown by the sundial) varies by about 1 part in a 100 plus or minus, and our calibrations of clocks must be made in terms of some more constant period if they are to be more accurate than this, hence the use of the "mean" solar day or alternatively the sidereal day. If more refined methods or theoretical considerations indicate that there is a definite error

¹ Mach, *Science of Mechanics*, Eng Trans., p. 124.

of a very small order of magnitude this can be corrected for where required.

It is necessary to notice that the successive corrections of a system of measurement can be made in two ways, one common and legitimate and one illegitimate. In the first case we may be assumed to set up a system of measures based on primitive experience which has some definite limit of accuracy, say for sake of argument 1 part in 100. Then with more refined instruments constructed and calibrated in accordance with our assumed system we find we can make measurements correct to 1 part in 10,000; having done this we find there is a constant or at least a calculable error of 1 part in 1000. This we can correct for so as to make our measurements really accurate to 1 part in 10,000. This process does not involve logically any abandonment of our old system because the correction introduced is not such as to affect the initial and fundamental measurements, as it is outside their limits of accuracy. This is what I consider the legitimate procedure which involves only refinement of the old system and not abandonment of it. That is to say our first rough measurements are assumed to give us laws which hold more exactly than our first observations can inform us about.

The illegitimate procedure would be if we corrected our measurements by an amount that would be detectable by our initial instrument. That would be sophistication of the whole process. For instance if we made our initial time measurements on which our system of mechanics was based correct to 1 part in 100 and then our consideration of the laws of mechanics induced us to make corrections amounting to 1 part in 10, that would simply mean that our initial time measurements were false, and our laws illusory, for a deviation of 1 part in 10 could have been detected. It might, of course, have happened that our laws were right but it would be a mere accident; we should never have had any reasonable ground for believing they were right.

Suppose we made a pendulum clock, basing our belief in its accuracy on the observed regularity of its ticking. Then with this clock we measured the length of the solar day and found we could detect variations in it, but could find none in the sidereal day, we should take the sidereal day as our standard and calibrate all our clocks by it, including our first clock. We would then go on to study the laws of falling bodies and the laws of impact and dynamics generally. Let us suppose that if there is any error the largest is in our time measurements so that all other errors are negligible in comparison and that this error is such that the laws of mechanics are only observed to be correct to 1 part in M . Now we have two alternatives, we may hold that the laws of mechanics are really only correct to 1 in M and our clocks are right, or that the laws really hold with a much greater accuracy and our clocks are inaccurate by some amount not more than 1 in M . If we adopt the first view no further advance in knowledge is possible, but if we adopt the second view we can use the laws of mechanics, on the assumption that they really hold with a much smaller limit of errors, to correct our clocks, by various devices practical and theoretical, and so greatly increase the accuracy of our results. The proof of the validity of this assumption will lie in the results obtained in consequence of making it, because if the assumption were false it is extremely unlikely that our time measurements would in practice turn out to be more accurate with our supposedly improved clocks. If our improved clocks show the laws to hold with a much higher degree of accuracy than 1 in M that is a confirmation of our belief.

This is the sort of way I believe the system of time measurements to have developed and I can see no other logical way. If we have no primitive time experience on which our construction is based or if we have sophisticated our time measurements so as to make Newton's laws of motion hold good, it appears to me that the

science of mechanics is nothing but an imposture on the human race. This I find it hard to believe. Human beings tend to dispute about all things that can be disputed, so that if our system of time-keeping has been imposed upon us by fraudulent physicists in the interests of the laws of mechanics, the universal conspiracy of agreement is nothing short of marvellous.

As against these contentions it may be argued, that if somebody were to invent an entirely new system of time measurements which was discrepant with our present system, but made the laws of nature very much simpler, there is no doubt it would be adopted and all our present clocks would be replaced by instruments working on a different principle. If this were to happen it is possible that in the course of centuries people would come to believe that the new clocks measured equal time intervals as primitively perceived and that the time system of the old clocks was quite artificial. It is not very difficult to persuade oneself that something is primitively perceived if only one tries hard enough. I do not think there is any complete reply to this argument. The best answer, as far as I can see, is that the new system would have to be based somewhere on some primitive perception of equality and inequality, and that in such a system time measurements would really not be used at all. We might call the quantities used times, velocities and accelerations, and so on, but they would be quite new conceptions under old names. The question is in any case somewhat speculative. And it does not touch the chief point I wish to make, that for the present system of mechanics time measurements of the ordinary sort are necessary and are logically prior. Nevertheless it is quite open to the physicist to use any sort of system of measurement of any sort of quantity that he cares to choose, and in every case the proof of the pudding will be in the eating. If the new system saves trouble and increases knowledge it will be legitimate. All that my argument is intended to show is, what is the logical

basis of the system of time measurement that is as a matter of fact in use at the present day.

The difficulties concerned with ascertaining simultaneity of times are really much less serious than those concerned with equal times. Everybody admits that within any given experience simultaneity of events is a matter of direct observation. The difficulties only begin when it is a question of correlating distant events which cannot be observed by one observer but can only be calculated by means of signals which take a finite time to travel. In the vast majority of observations that are not astronomical the difficulties are trivial. The errors involved in treating all terrestrial observers as in "the same place" are minute and negligible for most purposes. In astronomical observations the difficulties are apparent and the errors are mostly calculable on the classical theory. Real trouble for which Relativity considerations are required only occurs in the imaginary case of correlating observations made by observers moving at high speed relatively to one another. All questions of this sort may therefore be left on one side for the time being.

§ 27. Time intervals are, in practice, measured by means of lengths. A clock is a mechanical device, which, if we assume the correctness of the laws of mechanics, possesses some part moving with a uniform velocity. The ratio of any two times will then be the ratio of the distances travelled by the uniformly moving body. The notion of a velocity is thus required for the use of a clock, just as the notions of an area and the properties of a square are required for us to be able to measure the size of a square by means of the length of its side. It is I think a matter of indifference whether we consider these new notions to be derived by purely logical construction from lengths and times or to be arrived at empirically. I should suggest, however, that the idea of motion is really a new idea and cannot be obtained logically from space and time. But given the

idea of motion the notions of velocity and acceleration may be considered as capable of being arrived at analytically.

For Mechanics a number of additional ideas are needed: Force, Mass, Inertia, Momentum, Energy, and so on, to keep within the bounds of the classical Mechanics. Consider Mass: this is not a notion that can be logically derived from any of the ideas we have discussed already, which involve only space and time. It is indeed not easy to see how as a matter of history it arose. The Greek scientists, with the possible exception of Archimedes, do not seem to have had any idea resembling our notion of mass. It seems to have arisen, however, fairly quickly as the result of Galileo's investigations. The question of exactly how it arose is not of great importance, the important point is that it must have arisen as the result of actual experiment; as the result of finding that there was some new "thing" which could be measured. It was quite unnecessary for people to have any clear or definite idea of what mass is, all that was needed was some adequate and unambiguous way of measuring it. In what follows I am not attempting to give an account of how the idea actually did arise but simply to indicate in a very rough manner the kind of way in which it could arise.

We may assume that we have vaguely the idea of a force, simply as a 'push' and of a greater or less force as a greater or less 'push'. We know further that some bodies require a greater push than others in order to impart a given motion. Mass then can be conceived as what the bodies "possess" in different amounts which accounts for this difference in behaviour. We can therefore treat the force required to move a body as equivalent to the mass of the body and the acceleration (or change in velocity) produced, in other words we can write the equation $F = A \times M$. This may be treated as a definition of force in terms of mass or of mass in terms of force; it does not matter. The important

point is that it gives us a means of measuring masses. Accelerations we can measure directly in terms of lengths and times; not simply by a single measurement in most cases but by a series of measurements of the space travelled over and the time taken to do it. It may be assumed, whatever mass is, that identical bodies will have the same mass, that is to say bodies of the same substance and of the same volume will have the same masses. Thus any two pennies, or any two shillings, or any two sovereigns will have the same mass. And of course any given body will have the same mass at different times. From our equation then, by taking bodies of identical mass and giving them various accelerations we can measure forces, *i.e.*, keeping M constant we can find F in terms of A . Having done this we can then use our known forces for finding masses of bodies whose masses are not identical, using the same equation. This experiment is not an easy one to carry out directly but there are two easy ways which are logically independent and get round the difficulty.

Galileo's observations on bodies falling under gravity showed that in this case the acceleration was constant for all bodies so that for gravity Force is proportional to Mass. The force of gravity can be measured statically by means of the Balance and measured easily and accurately. In order to use the Balance we may suppose either that we can deduce the properties of the lever on general grounds as Archimedes did, or that we have observed that as a matter of fact a balance with equal arms is in equilibrium when two identical bodies are placed one in each pan. On either assumption we can find the masses of bodies by the process of weighing. The other method that can be used is not nearly so useful in practice as this, nor I think so satisfactory theoretically; it involves the application of the laws of impact. If two inelastic bodies moving freely at constant velocity in opposite directions collide they may

bring each other to rest or may move together at a slower velocity in one direction or the other. Now the "properties" possessed by the bodies which are relevant are velocity and mass and the capacity of one body to stop another body must be a function of the velocity and the mass. The early physicists appear to have taken it as self-evident that the function in question was the simplest possible, the product of the mass and the velocity, which they called the Momentum. It is very difficult now to understand why it should have been thought to be self-evident. But this does not really matter because experiment will show that no other function meets the case. That is to say we can take different bodies of the same substance and on the assumption that mass is proportional to volume, which can be measured, we can find what relative velocities are required to bring different masses to rest after impact. If the bodies come to rest we know that the property sought is equal in both bodies and hence we can find empirically the relation $m_1v_1 = m_2v_2$ where m_1 and m_2 are the masses and v_1 and v_2 the velocities of the bodies. In practice it will be easiest to do the experiments by assuming the laws of falling bodies or the properties of the pendulum and measuring velocities indirectly in that way, but the experiments can be done without any appeal to gravitation. By this method it will be found that we get the same values for the mass of any body as by weighing. The importance of impact experiments lies in the fact that they show that inertia-mass and gravitational mass are the same. We could also study experimentally the laws of elastic impact and similar conclusions would be reached; but this case is rather more complex.

It will be seen that in order to investigate the question of the masses of bodies we must start with some sort of idea of what mass is, and this is not the sort of idea that can be considered as immediately given in experience in any way. It is essentially the result of reflection

on the facts of experience. Given the idea it is not necessary that it be conceived precisely or exactly defined ; it may be as vague as you like. What is necessary is that it should be capable of measurement and that we should get consistent and valid results from our measurements, and it is not the case that a clear or exact idea is required for this. It is necessary that the idea be adequate to the problems it is intended to solve, but nothing further is necessary. An adequate idea of number for the purposes of arithmetic is an idea that enables the possessor to carry out successfully ordinary arithmetical operations. A clear and accurate and definite idea of number will only be possessed by those, I take it, who have read and digested nearly the whole of *Principia Mathematica*, and it is important to note that they will not, in all probability, be able to do arithmetical calculations any better after, than before reading that work. That is not to say that clear and accurate ideas are of no use. They are of use because they are adequate for all purposes to which they can be put, whereas ideas that are not so clear and accurate are only adequate for certain purposes, and in special cases may lead to error. For instance the ordinary person's idea of number is adequate for all ordinary purposes but will be found inadequate when he tries to deal with infinite numbers and for this purpose greater clarity and accuracy are needed. In the case of mass the test of the adequacy of the idea is whether it can be measured in any way and whether the results of such measurements are consistent and satisfactory in enabling us to find out more about the external world. If at any point discrepancies and difficulties arise, if the idea is found inadequate, it must be revised or modified in some way to meet the case, and in this process it will become more clear and distinct owing to the clarifying effect of criticism and discussion. On the whole the idea of mass has proved adequate to the demands put upon it, and very little revision has been required. Probably

if it had been less adequate we should know more about it. Among the most adequate ideas we have are space and time, which are prehistoric in origin, and are the very reverse of clear and accurate, simply because until very recent times no criticism has been required.

In order to be able to make use of a new idea such as mass we must be able to construct equations in which it stands as a function of old ideas and which enable us to make the necessary measurements. To construct the equations at all we must have some basis of both theory and observation. For instance, it was assumed above that identical bodies had the same mass and that for bodies of the same substance the mass was proportional to the volume, and finally that the mass of a body was constant in spite of changes of other properties. These statements cannot be considered as self-evident or necessary *a priori* or as the direct result of observation. The proper way of looking at the matter is to regard them as part of the definition of mass put forward as a hypothesis. We may say: "Let there be a property of bodies such that for any body it is constant at all times and for bodies of the same substance it is proportional to the volume and such that when a force is applied the acceleration multiplied by this new quantity equals the force; call this property Mass." Then on this basis we may investigate and measure the masses of bodies and the success of our operations is the measure of the value of the hypothesis. But of course in making such a hypothesis in the first instance we must not so frame our definition as to violate any known property of bodies.

For any advance in scientific knowledge we require both the proper apparatus of ideas and the necessary methods of observation and the most valuable combination of theory and experiment is that involved in the process of measurement. Measurement is on the one hand an extension of the process of classification, and on the other is a process of induction. As regards the

ideas used they must necessarily be the right sort of ideas though not necessarily clear and accurately defined. One of the characteristics of the great scientific geniuses is their capacity for inventing the right sort of new ideas. There are no rules for this process of invention. If there were, anybody could learn to be a Newton or a Faraday.

It will be well before leaving the subject of measurement to consider shortly measurements of another sort. So far I have dealt with the measurements of quantities; there are also Intensities or Potentials that can be measured. This is rather a more complex business. In the case of quantities we have either a primitive perception of equality and inequality or else judgments as to equality and inequality may be made derivatively from primitive perception; at any rate we can legitimately say that we observe equality and inequality. In the case of intensities the process is not so simple. We can observe that one temperature is higher than another but we cannot say by observation what are equal increments of temperature. An increment of temperature is not itself a temperature as an increment of weight is a weight. It is only by convention that it can be expressed as a difference of two temperatures. To measure temperatures then we have to set up a conventional scale. Observing that bodies expand with rise of temperature we select some convenient substance as a thermometric substance (air or mercury) and say that equal increases in volume are due to equal rises in temperature. Hence with two arbitrarily selected points of reference a numerical scale of temperatures can be constructed. The whole process is arbitrary and conventional and is hardly yet measurement in the strict sense of the term. But it is assumed that if any illegitimate assumptions have been made they will betray themselves in the course of working.

The final step in the development of temperature measurement is the definition of the Absolute Zero and

of equal increments of temperature in mechanical terms with the help of thermo-dynamic theory and Joule's Equivalent. In this way temperature is made completely metrical and independent of arbitrary assumptions and the vagaries of any particular thermometric substance. Of course in practice a thermometric substance will still be used, but we shall know its defects and be able to correct for them.¹

¹ I have not said anything on the subject of the system of units of measurements or of the standards in use, because this is a technical matter. The fact that the standards of measurement are particular and arbitrary, e.g. the standard of weight is a particular lump of metal kept in a particular place, is well recognized nowadays and hardly needs emphasis. The matter has been well treated by Campbell (*Physics The Elements*, p. 395.)

CHAPTER VI

Theories

§ 28. THE verification of Laws ultimately involves an appeal to experience but this appeal may be indirect. That is to say the appeal may be first to laws of less generality and only through them or through a whole series of laws back to experience. The word 'fact' is used very ambiguously; it is usually supposed to mean a particular fact, that is to say an event; but it is often used to mean simply an assertion about the external world to which great confidence is attached whether it be particular or general. Thus we say it is a fact that Socrates died at Athens in the year 399 B.C., but we also say it is a fact that Benzene boils at 80° C. The first proposition asserts the occurrence of an event, the second the truth of a law. This ambiguity may be regrettable but it is so deeply embedded in language as to be beyond remedy; actually it does not lead to any serious confusion. When people say they appeal to facts they usually actually appeal to well established laws but they often think they mean an appeal to sense experience, to particular events. If you examine the statements of "fact" in any scientific work you will find that almost invariably it is laws that are referred to but laws of little generality and high probability. The reason for calling them facts is that they are capable of immediate verification by experience, of verification that is by direct observation with the intervention of the minimum amount of theory beyond that involved in the construction of the instruments of observation. This usage is quite reasonable. An event that does not form an instance of a law (or of its con-

tradicitory) is of no scientific interest and is not worthy of record. For purposes of communication it is only the law and not the particular event that can be dealt with. The distinguishing character then of the kind of law that is properly called a fact is simply its immediate relation to experience. Of course this use leads to some abuse in common speech so that 'theory' comes to mean simply what other people believe, 'fact' what I believe.

I have not thought it necessary to distinguish precisely between the meaning of Law, Theory, and Hypothesis, in this respect following common usage. Generally laws are supposed to be generalizations of high probability and hypotheses of lower probability, the word also meaning what its derivation suggests. Theories occupy an intermediate position with the additional assumption that they are more general than laws. All these terms are used quite loosely; they are all generalizations. The particular name given to a generalization will depend not on the nature of the generalization but upon how we are regarding it at the moment. I have mentioned this point because Campbell¹ uses Law, Theory, and Hypothesis in special technical senses. His treatment appears to me highly artificial and even absurd, but in one respect it is of importance. Laws as he uses the term are generalizations of small generality obtained more or less directly from experience. Theories are derived from laws but deal largely with hypothetical entities and relations not directly discoverable from experience. On the whole it is a useful distinction as enabling one to see how near to or remote from experience any generalization is. It is convenient then to use the name fact for the first and simplest generalizations from experience; laws for the combination of facts into wider generalizations, but into generalizations that still stick close to experience and describe only perceptible relations or properties.

¹ *Physics The Elements*, chs V and VI

Theory is a still more general statement involving unverifiable hypotheses. Thus the individual and reproducible relations observed between pressures, volumes, and temperatures of gases are the facts. The generalization of these into the relation $PV=RT$ is the Gas Law. The theory is the Kinetic Theory which is based upon a hypothesis as to the structure of the minute imperceptible parts of the gas.

§ 29. In our previous discussion of laws we have been concerned chiefly with a fairly simple type; it is now necessary to consider the more complex types that enter into the construction of scientific theory. We may conveniently distinguish three types of law corresponding to the three types of object of Whitehead's classification. These are first the simplest kind of laws that describe the relations of *sense objects* and that go to make up the theory of *perceptual objects*. It is only in the elementary classificatory stage of science that these laws come in. The next kind is that concerned with the relations of perceptual objects and these are the old-fashioned causal laws. At this level too we have numerical laws, which are a refinement of the causal laws. Lastly we have the laws relating scientific objects, the theories of modern science. These loom largest in the eyes both of men of science and of the general public, and I hope I shall be pardoned if I say that they are of less importance than the simple numerical laws on which they are based—if they are based on anything.

It is a grave difficulty in the analytical treatment of scientific ideas that the simple laws are so vaguely conceived that analysis is difficult and the theories that are precisely formulated are often put forward as dogmas and the evidence in favour of them has sunk into oblivion or into the realm of myth. The nerve-shattering effect of the Theory of Relativity on many physicists is due to its compelling them to drag their mythologies out into the light of day; and very quaint

some of them look too. It would be an interesting task to examine carefully any ordinary text book of Physics or Chemistry and try to unearth all the myths to be found embedded there—the experiments that nobody has done or could do, the sophistries that support theories, and the sheer dogmatic assertion unsupported by even a pretence of evidence. There is much there as fantastic as primitive folklore, though not so picturesque.

Probably a certain amount of fiction is inevitable in any theory, but it is important to keep the amount as small as possible. There have always been men of science of a severely logical cast of mind who have done their best to strip away the mythological parts of scientific theory, but they have had a hard task and have not usually gained much sympathy. Generally speaking if we rid a theory as far as possible of fictitious elements it loses the simplicity and neatness that endears it to all. We have to make elaborate reservations and draw minute distinctions; we have to use clumsy terminology and laborious mathematics, difficult to understand. And the advantage gained may be miserably small; nothing but a minute correction of the old formula in some out of the way instance. No wonder mythology is popular.

At this point we have to consider a very serious confusion that has arisen between what is abstract and what is fictitious. By a fiction or a myth I mean simply what is not true, or rather what, though it might conceivably be true, we have no earthly reason to think so. Whereas an abstraction is something considered in isolation from elements that accompany it in nature. All knowledge inevitably involves abstraction and there is no vice at all in abstractions as long as we know they are abstract, but fictions are vicious. There are philosophers, I am aware, who consider that all abstraction implies fiction and falsehood; but I have no patience with them. Their views if correct would make

knowledge impossible and plunge us in a timorous and ridiculous scepticism in which we dared distinguish nothing from anything else for fear of saying what was not true. If these men were right an oyster would be as wise as Socrates. An oyster analyses and distinguishes nothing. It contemplates the Absolute. Socrates, poor man, was always analysing and distinguishing and abstracting and getting into all kinds of trouble. Those who prefer the life of the oyster we shall not interfere with, but for ourselves let us follow the example of Socrates, distinguishing carefully, as he did, between what is fictitious and what is abstract, and rejecting the former but cherishing the latter.

Abstraction then is a perfectly legitimate process and it does not follow that what is most abstract is most remote from reality, how remote it is depends upon the amount of fiction that has crept into the process of analysis and construction by which the abstract idea has been reached. Fictitious elements in theoretical ideas are sometimes excused because they lend concreteness to what would otherwise be painfully abstract; and there is a certain amount of sense in this. Whatever has in it some concrete element, some degree of particularity, is more readily grasped than what is purely abstract. It can be imagined whereas the other cannot. This probably explains why sometimes palpably fictitious theories have been more fruitful in producing new discoveries than perfectly correct but quite abstract formulæ; they appeal to the imagination. In fact, as long as the fictitious part of a theory is recognized as what it is, and merely an aid to human weakness, it cannot do much harm and may even be useful. The evil of fictions is the risk of their being unrecognized and mistaken for truth. As Plato says, it is the lie in the soul that matters not the external falsehood.

So much then by way of warning before we consider the status and relations of various types of law and

theory. Perhaps a further warning is necessary, that in such a discussion it is impossible to avoid being biased by the particular state of scientific theories at the moment.

§ 30. The enumeration and definition of the characters that go to make up a substance or any type of body consists in the first instance of the assertion of a set of laws holding among sense objects. When we say that silver is a substance having a white "metallic" lustre, that it is heavy and ductile and malleable and a good conductor of heat and so on, we mean to assert as laws the association of a certain shiny whiteness, a certain coldness, hardness, and heaviness, and further—though these are more complex laws—that these properties are associated with a certain behaviour when beaten with a hammer and heated and so on. The second set of laws are of the causal type and assume the existence of a whole body of simpler laws of the first type that go to make up the notion of perceptual objects. Thus the notion of malleability implies the notion of a hammer and its functions, the result of generalizations about hard tenacious and massive bodies. Let us consider first the simpler sense-object laws only, which are sufficient by themselves to form some notion of "bodies" and the beginnings of a theory of perceptual objects.

The relations of sense objects are somewhat vague and uncertain, particularly in so far as they depend upon qualities of perception. There is something private about a "*quale*" and curiously resistant to generalization. From appearance alone we cannot be sure of distinguishing silver from other metals in every case. It will never be confused with copper or gold but it may be mistaken for tin. Of course if we have a specimen of silver and one of tin side by side we can see a difference in the colours and in the case of thin material which can be bent tin can be recognised by its peculiar dry crackle—the so-called "cry" of tin. But in

the case of impure metals and alloys all the criteria may fail. The clever craftsman can make an alloy of base metals of the exact colour of gold and can mix a considerable amount of copper with his silver without altering the colour. All the laws of qualities are vague and liable to exceptions. But even at this stage there are certain generalizations that can be made with greater precision. Of the shapes and sizes and motions of bodies we can find certain very constant laws. A body perceived from different places has different appearances but these appearances change, as regards shape and size, in a perfectly regular way according as we change our relative position. The correlations, too, of seen and felt shapes are very constant and exact when made in the proper way. In this respect then of spatial relations and properties we can find quite constant laws of the simplest type. It is of course the constancy and precision of these laws that lead to the belief that the geometrical properties of bodies are somehow more real than the others, so that the same theorizing that transferred colours and sounds to the mind of the percipient was not applied to shapes and sizes which were, by a legal fiction, allowed to belong to the external world.

When we come to consider the more complex type of laws holding between bodies as such and not merely between sensible qualities of bodies, the laws that are traditionally called causal, we find again that most of them are very vague and uncertain, but that there are a few which admit of greater certainty and precision, namely those that are the result of measurement.

The theory of perceptual objects and of causal laws that is accepted now-a-days is a very complex structure and has been greatly altered under the influence of scientific criticism. It has not developed in a systematic way but each objection has been met as it turned up without regard to the symmetry or consistency of the whole theory. This makes it extremely difficult for

anybody to say exactly what we do or do not believe about perceptual objects and causal laws, because our belief depends upon how much we are under the influence of scientific theory or philosophical analysis or are merely primitive and naive. Probably our views are oscillating and contradictory.

A perceptual object should be the class of its appearances, as Russell was the first to point out, but in practice we do not attain to such a height of philosophical exactness. Instead we treat some one selected appearance, a complex of sense objects visual and tactical, as symbolizing it. When we speak of a chair what our imagination presents to us is some particular appearance of the chair, a perceptual symbol of the chair. The method by which the symbol is selected may be more or less rational but it may be and usually is arbitrary and fantastic; it may even be formed inconsistently by including appearances that could not appear together. Thus the naive perceptual symbol for a human face is a profile with one or even two eyes seen in full. This particular symbol may be seen in the drawings of primitive people and children. Probably even our adult and educated symbols are not much better. One at least of the causes of the unsatisfactoriness of causal laws is that it is not clearly recognized whether the law holds as between the perceptual symbols of the bodies or between the bodies themselves, the whole class of appearances. The latter way of regarding a causal law is undoubtedly correct as the former is incorrect, but it would make the formulation of the law extremely complex. If the law is treated as a relation between the symbols it will be simple enough but extremely uncertain. This difficulty persists as long as we consider only unanalysed qualitative resemblances of bodies. Measurement gets over the difficulties to a large extent. In measurement the bulk of the perceptible qualities of a body are neglected and only the geometrical ones considered. These are dis-

tinguished, as already mentioned, by the simplicity and constancy of their relations. Bodies are then compared in respect of one geometrical property under certain standard conditions as, for instance, by juxtaposition. The numerical ratio that expresses the result then symbolizes the body, as the ordinary perceptual symbol did before. It is of course more abstract and meagre than the perceptual symbol but it has the advantage of being derived by a rational process and not arbitrarily.

The numerical laws obtained by this means are the basis of all exact knowledge. It is worth noting in passing that the sciences that have not yet got beyond the "classificatory" stage are able to advance and make their methods precise in proportion as they are able to base their methods upon the comparison of geometrical properties of bodies. As Whitehead has remarked, biological classification is a branch of geometry. Simple numerical laws, however, are not enough for scientific theory; they are numerous, special, and complex; and even though more exact than other laws they do not hold absolutely invariably or exactly.

Theoretical developments that go beyond simple numerical laws and causal laws are intended to introduce several improvements. They are intended to make laws (1) more general (2) more exact (3) to avoid certain complications that make analysis of the transactions of ordinary bodies difficult. These complications are of two sorts, material and formal. The material complications are the presence of effects and processes which are not readily reduced to order and incorporated in laws such as those due to friction and the imperfect elasticity of bodies in Mechanics. It is much easier to deal mathematically with systems that are frictionless and perfectly elastic, and so on, therefore there is an inducement to look for them in Nature in case there are any such. It is also considered desirable to deal with

entities that are conserved, and not variable like ordinary properties of bodies ; though the theoretical importance of conservation is probably much exaggerated.

The formal complications are due to the difficulty of treating mathematically of the transactions of bodies of finite volume as a whole. It is found necessary to consider the large scale transactions as compounded of minute transactions. Attention is therefore focused on the supposed minute parts of bodies which are not only destined to provide simpler equations but are also supposed to be perfectly frictionless, perfectly elastic and perfectly this, that and the other according to taste. The immense success of the kinetic theory of gases has undoubtedly enhanced the prestige of "microscopic" methods so that every branch of science that utilizes physical method has got involved in the theory. Even Thermodynamical theory that professedly treats only of large scale measurable effects without postulating any microscopical mechanism does actually derive its cogency from microscopical theory. That is to say the Second Law of Thermodynamics, as long as only macroscopic processes are considered, appears as a rather precarious empirical generalization and is open to theoretical objections, which physicists were not slow to bring against it. But if it is considered as the result of a certain type of microscopic mechanism it has a rational basis. It becomes equivalent to the assertion that certain configurations of a microscopic system such as that postulated by the kinetic theory are more probable than others and that the more probable tend to replace the less probable.

I do not wish to enter into the arena of controversy in matters of physical theory, and even if I were willing I am not competent to do so. All I wish to do is to indicate what seem to me the leading characteristics of modern theory that are of importance from the point of view of method. Modern physics is inevitably wedded to microscopic theory and nobody can ignore the success

that has attended the use of it. Nobody on the other hand should ignore the very large amount of fiction that has been incorporated in it. The efforts to eliminate the element of fiction are of two sorts—experimental and theoretical.

The experimental method consists in accepting the assumption as to the microscopic composition of bodies absolutely literally, working out the consequences and then seeing what experimental evidence can be got in their favour. This method has met with great success and has established the microscopic theory very firmly. The early mathematical treatment of the kinetic theory assumed gas molecules to be massive points or perfectly elastic spheres. Now they are supposed to have shape and volume (in a special sense) and not necessarily to be perfectly elastic or perfectly anything. Not only can their numbers and velocities be measured but their shapes, and it is even possible to find exactly how the atoms, and their component electrons and nuclei, are arranged in a crystal.¹ It is noteworthy that the branch of Physical Chemistry which is most stagnant and unproductive at the present time and in which the greatest number of discrepancies and anomalies are complained of is the theory of the *liquid state* and of *solution*. It is just here that microscopical theory is most backward and most difficult.

The other method mentioned, the theoretical, which is unfashionable at the present moment, is to build up a theory without assumptions as to the actual minute composition of bodies. If any such considerations are introduced at all they are brought in ostensibly as mathematical figments intended to simplify calculation and not as representing any real entity. An extraordinarily interesting attempt to develop chemical theory

¹ For the recent developments of the Kinetic Theory, reference may be made to Jeans, *Dynamical Theory of Gases* and Perrin, *Les Atomes*, on the structure of crystals to H L and W L Bragg, *X-Rays and Crystal Structure*. For the investigation of the dimensions and shapes of molecules, see Adam, *Proc. Roy. Soc.*, Vol 101, p 452, 1922.

without the assumption of atoms and molecules was made some years ago by Ostwald¹ and has not received the attention it deserves. Ostwald pointed out that there was no chemical evidence for the existence of atoms and very little evidence of any sort (at that time) and therefore chemists ought to base their theory on actual chemical procedure and on the simplest and most general physical laws. Starting from the Laws of Thermodynamics and the Phase Rule he showed how all the fundamental chemical laws could be developed independent of the atomic theory. Considerations apparently borrowed from the atomic theory such as molecular and atomic weights were introduced as being merely convenient fictions.

It is one of the strong points of the type of argument exemplified by Ostwald that theoretical considerations that have an element of fiction are introduced avowedly as fictions. On the other hand it is never easy for them to explain how the use of anything that is purely fictitious can lead to correct results. This is the strong point of the opposite, or mythological, school that if their myths are fruitful in correct results and new discoveries they are so far justified. In fact the controversy can be summed up fairly well, I think, by saying that the mythological school have usually made more discoveries than the strict rationalists but that the rationalist criticism has cleared away absurdities. It cannot be denied that at the present moment microscopical theory is in an immensely strong position.

§ 31. It is customary to assert, or to deny, according to taste, that the end and object of scientific investigation is to find a mechanical explanation of everything in Nature. The asserters and deniers never seem to have time to explain what they mean by mechanical explanation, beyond dropping a hint that it has something to

¹ Faraday Lecture of 1904 (*Jl. Chem. Soc.*, Vol. 85, *Trans.*, p. 506) The subject is treated at greater length in his *Fundamental Principles of Chemistry*, transl. Morse, 1909

do with the science of Mechanics, for which the asserters express a great fondness, but which has an irritating effect on the deniers. The only author, as far as I am aware, who has ever paused to consider what the phrase means finds that it has at least five different meanings; an excessive allowance. Therefore before we join either party it will be as well to consider, with Broad¹, what the various meanings are.

Difficulties underlying the use of the word "explanation" need not detain us. Its meaning in this case is fairly clear. It means to describe special laws of nature in fresh terms which are more general or better understood or more familiar than the original ones. Thus we say the Kinetic Theory provides a mechanical explanation of the behaviour of gases, because it displays the ordinary gas laws as special cases of more general laws, namely, as the macroscopic result of the motions and elastic impact of microscopic parts. The real difficulties come in when we consider what is meant by mechanics, what laws, that is to say, are mechanical and what are not. The distinctions therefore are not quibbles but involve important scientific principles.

Now in the case of the gas laws, many of the observed properties of gases are mechanical in the sense that they are measured in terms of the fundamental mechanical units of length, time and mass or functions of these. Pressures, densities, volumes, rates of diffusion and so on are all expressed in C.G.S. units and are so far mechanical. But temperature is not mechanical as it has its own special system of units. Therefore any law of gases which includes temperature in its expression is not obviously mechanical. Moreover, the formal expression of the gas laws cannot be twisted into a form in which it resembles the Laws of Motion or any other mechanical law.

What the Kinetic Theory does is to show that the measurable macroscopic properties of gases including

¹ *Proc. Arist. Soc.*, Jan 1919, p 86

temperature are the result of microscopic transactions which are mechanical in a strict sense, for they can be completely described in terms of the Laws of Motion and the Laws of Impact. This is what may be called Microscopic Mechanical Explanation.

The importance of the Laws of Motion is that they are the most general of all laws of physics. They apply to all bodies because they are concerned only with what all bodies have in common, volume, figure, motion and mass. Now in any isolated system (leaving out of account a few special cases) mass is constant, so that the only variables are Time and Spatial Co-ordinates, Geometrical Properties and Relations.

Any system then will be completely defined from the point of view of the laws of motion when we know the values of :—

- (1) The Geometrical Properties and Relations
- (2) The Kinetic Energy—a function of Mass and Geometrical Relations
- (3) The External Forces acting on the system
- (4) Certain universal constants, like G.

The external forces may be of many different types. They may be due to gravitation, to Impacts or to other causes—Electrical, Magnetic or Chemical effects or to Radiation. Gravitational effects are, as Broad argues, considered as mechanical in a special sense, because they do not involve the recognition of any qualitative differences among bodies and they are specially amenable to mathematical treatment. The same argument applies to the Laws of Impact in so far as we can consider impact as elastic, so that different coefficients of elasticity less than unity need not be considered. The treatment of Electro-magnetic and of Chemical forces presents greater difficulties analytically, and involves, in the present state of knowledge, the admission of irreducible qualitative distinctions among bodies. The aim of a rigid mechanical treatment of problems

is to eliminate all consideration of differences of kind among bodies and to reduce all variables whatsoever, including those due to 'external forces,' to geometrical ones and to admit no constants except universal constants and mass.

It must be remembered at this point that theoretical mechanics always tend to deal with "ideal" systems in which certain effects like Friction, Cohesion, Viscosity, Surface Tension, etc., whose analytic treatment is difficult, are left out of account. Because in most large scale transactions effects of this sort cannot be left out of account without grave error, it is assumed, as a matter of courtesy, that they are not apparent in microscopic transactions. As some at least of them, such as surface tension, will on the microscopic scale be resolved into attractions between molecules, this procedure is justified to a large extent. Certain difficulties of this type, however, still remain. As Jeans points out the assumption that no kinetic energy is lost in collisions of gas molecules is quite gratuitous and is only justified because the most careful large scale measurements show that any such loss must be very small. This is by way of warning against the assumption that is too common, that even if mechanical theory cannot be applied to macroscopic systems, except as a matter of approximations, all troubles cease as soon as we turn to microscopic systems.

Most people when they speak of Mechanical Explanation are thinking of Microscopic Mechanical Explanation whereby measurable effects not *prima facie* mechanical are described in mechanical terms microscopically. It is as well to keep in mind, therefore, what Broad points out, that there are at least three different varieties of microscopic mechanical explanation, which differ according to the strictness with which the terms admitted into the equations used are restricted to purely mechanical variables and constants and how far special laws and special properties of bodies are allowed consideration.

In order to see the results of microscopic mechanical explanation let us return to a consideration of the properties of gases. One obvious respect in which the Kinetic Theory made the study of gases more mechanical was that it enabled temperature measurements to be expressed in terms of C.G.S. units; what appears macroscopically as temperature being equivalent microscopically to the average Kinetic Energy of the particles. Does this absolve us from the necessity of making temperature measurements with thermometers in the ordinary way? The answer naturally is, No.

Let us suppose that there was a race of Physicists, in Mars perhaps, who had no sensations of heat and cold and consequently no primitive idea of temperature. When they were studying the properties of gases they would notice a curious thing happening as the result of compressing or expanding a gas. When a gas was compressed suddenly by a given amount the pressure obtained would be higher than that obtained by compressing it very slowly to the same extent. Moreover the pressure obtained by rapid compression would slowly sink to the value obtained by slow compression. Sooner or later some genius would explain this anomaly. He would explain that when a gas is compressed work is done on it so that its energy is increased. Therefore it must be assumed that there exists a new form of potential energy which can be transferred from one body to another. All bodies would be supposed to possess some of it and there would be a tendency for it to pass from a region of higher to a region of lower potential at a rate varying with the character of the body. If we take a cylinder with a piston containing any gas and compress it suddenly by a given amount the pressure will increase by p_1 , if we compress by the same amount infinitely slowly there will be a smaller rise p_2 . The difference p_1-p_2 will be a measure of the increment of this form of energy obtained by compression. Similarly by expansion we can measure decrements of energy.

A closed rigid vessel containing a gas and provided with a pressure gauge can be used as a measure of the potential of surrounding bodies in respect to this form of energy, the pressure varying according to the potential at which the vessel is kept. In fact a gas thermometer would have been invented by the Martians although they had no experience of heat and cold, in order to explain anomalies in the behaviour of gases.

Once they had obtained a knowledge of temperatures and instruments for measuring it they could develop the Kinetic Theory. Before that, while temperature was still obscurely mixed up with pressure and volume the application of the Kinetic Theory would be impossible or at any rate very difficult and uncertain. It is extremely unlikely that it would be of any assistance in elucidating the phenomena of temperature.

However, the notion of temperature is arrived at, whether directly from sensation as we get it or indirectly by means of physical theories as our natural philosophers of Mars did, the instruments for measuring it must be the same in principle. No amount of mechanical explanation and theory can liberate us from the necessity of using thermometers. The way we arrive at the method of constructing and using thermometers may alter the names we give to what we measure but it will not alter the facts. The Martian physicists, who lacking the sense of heat and cold, have been more strictly "mechanical" in their methods, will not have been helped but hindered in their investigations.

It may be argued that the Kinetic Theory enables us to calculate all the properties of a gas from a knowledge of the numbers, masses and velocities of the molecules, and therefore there is no need of macroscopic measurements. As against this it must be pointed out: (1) the numbers, masses and velocities, can only be obtained by calculation from macroscopic measurements, (2) the values obtained are not very accurate so that macroscopic properties calculated back from them

are not as accurately found as they can be by direct measurement, (3) the deductions from the Kinetic Theory are not simple or easy at the best and if the gas is not 'perfect' they become very difficult or even impossible. However much microscopic theory we indulge in our accurate knowledge of facts is obtained by macroscopic measurements and by these only. The fundamental laws of Physics are and must remain the empirical laws that are discovered by measurement. Microscopic mechanical theory does no more than correlate them ; its use is that of a calculus. It enables us to substitute one observation for many, to check one observation against another, so that the accuracy of knowledge is increased and its range extended. Further it puts into our hands a most powerful weapon of mathematical analysis so that all sorts of remote and otherwise improbable consequences can be calculated. The real virtues of mechanical theory are so great that there is no need to attribute to it virtues it does not possess.

It must be mentioned that while the Newtonian laws of motion are supposed adequately to describe transactions between molecules there is no good reason as yet to believe that they cover transactions within the molecule. These may follow entirely different laws. In particular the laws of intra-molecular events may not be capable of expression as differential equations at all, but only as relations of integers. As microscopical analysis of bodies proceeds it may be seen that the Newtonian laws do not hold quite exactly at any stage and not at all at the last stage. This is all a matter of pure speculation, of course, but such speculation may be useful as showing that the Equations of Lagrange, valuable as they are, are not yet worthy of divine honours. Physicists seem to have a curious fondness for putting something in the position of the Demiurge of Plato, the Laws of Motion or the Ether or the Velocity of Light, which appears to be latest candidate for the post.

When special laws are explained in the sense of being considered as special cases of more general laws it is necessary to be careful about the business to see that the advantage gained is not illusory. A special case is usually special because it is specially complicated. Although we may know vaguely that it is a special case of more general laws we may be entirely unable to make any use of this knowledge. The difficulty may be because (1) the special laws are not yet well enough known, or, (2) because the mathematical treatment is too difficult. The Kinetic Theory cannot be applied to liquids, except in a few special cases, for the second reason, that the conditions are such as to make mathematical analysis difficult. The special laws of liquids are, however, quite sufficiently well known to make such an application profitable if it were feasible. On the other hand the application of mechanical or other physical theory to meteorological processes is hindered by the poorness of the data available, that is to say the special laws of the weather are not sufficiently well known to give the application of general physical theory any great value.

It must be remembered too that the information provided by very general and abstract laws is in most cases negative; they tell us what things cannot happen, but only in special cases and within certain limits what will happen. Mechanical laws supply the greatest amount of positive information in the case of machines, because machines are systems which are specially constructed so as to reduce the possible varieties of motion within very narrow limits. Then on the application of external force by human agency (directly or indirectly) the motion takes place within those limits. For instance if you wind up a clock it will go, as a general rule, and cannot possibly go backwards. If it does go, its speed will not exceed certain limits. But it is not contrary to the laws of mechanics for it to go fast or slow or not at all. Lastly it will not go unless it is wound up. These considerations are so trivial and obvious that it is easy

to forget about them. However exactly laws are capable of predicting the course of events, the amount of prediction actually obtained depends upon the extent and precision of the observations made. It is very easy to cherish a vague idea that if only your laws are general enough it is not necessary to observe at all. On the contrary the more general the laws the more precise and extensive must be the observations in order to get any good out of them.

The question as to the standing and importance of the various forms of microscopical mechanism is largely a technical one. Any views held on the subject are bound to be coloured by the particular stage of development of scientific theory reached at the moment; so that not much can be said with any great confidence about it. There is, however, another matter, another possible sense of mechanical explanation of greater importance and about which something can be said with greater probability. Owing to the characteristics of our methods of measurement all our actual observations are of geometrical variables. We call the result of our measurement temperature or electric potential or weight or what not, but what we observe is a length or an angle. This point needs no further labouring. Broad points out that if we treat our instruments of measurement as themselves part of the system of bodies studied, our only variables will be geometrical and we should have accomplished a special kind of mechanical explanation which is of universal application. Broad calls this Metrical Macroscopic Mechanism. The result of such a method of expressing results would be that all processes and states of any system to which measurement can be applied could be expressed in terms of (1) Geometrical variables, (2) Certain special constants depending on the nature of the instruments and other parts of the system, (3) Certain universal constants. Doubtless the equations obtained could be cast into a form similar to the laws of motion.

Whether it would ever be profitable to work out the results of investigation in this way is difficult to say. Certainly it has not been tried. However that may be, the notion is an important one, because we know that outside the region of measurement there is little reliable knowledge to be found ; and we know too the peculiarly important position of the spatial and temporal relations of bodies.

§ 32. We must now turn to consider a question about which much ink has been spilt, namely the controversy about the applicability of "mechanical explanation" to biological processes and the sufficiency of such explanations to solve the problems presented to us by living things. The methods of controversy adopted remind me of the tactics of certain marine creatures — Holothurians — when they are attacked. These ingenious animals, who have no obvious means of defence, eject from their interior vast masses of soft stringy material, and so entangle and bemuse their adversary with these Laocoön-coils that they are able to retire to some place of safety and reconstruct their internal economy at leisure. So do the Vitalists and Mechanists both, when challenged on these matters, produce from the interior of their minds a vast mass of soft and stringy metaphysics with which to entangle the questioner and keep him quiet till they have retired from sight into their den of impenetrable ignorance. For so little is known about living things that anybody can imagine anything he likes. Most of the problems have not even been stated, much less solved, and it is absurdly easy to say that the solutions of these unknown problems will be this, that, and the other, or that there will be no solution at all.

I think anybody who has seriously investigated living things, not to prove any metaphysical system, but as far as possible with an open mind and in a spirit of pure enquiry, comes to much the same opinion in the end. He sees how curiously dependent life is on

the physical laws of its environment and how it is, as it were, soaked in them ; and yet how independent it is in some respects and how unlike anything that is not living. This idea has been well expressed by Professor A. V. Hill,¹ and I cannot do better than quote his words.

"The living creature is a very strange thing. Here it is in a physical, material, mechanical, chemical universe showing the most extreme dependence on physical conditions. It dies if it be frozen, or boiled, or deprived of food ; it does not like strong acids, or ultra-violet light, or x-rays, or trivalent positive ions ; its properties depend to a most extraordinary degree on those of water ; it conducts electricity ; it obeys the laws of motion, of the conservation of energy, of the conservation of mass ; and yet at the same time it shows, in its simplest forms, a complexity, an apparent purposefulness, an individuality, defying any physical hypothesis. One could treat a chemical theory of consciousness, or a physical theory of heredity, or a mechanical theory of intelligence, only one degree more seriously than the instruments recently designed by Mr Edison for facilitating telephonic communication with the next world. And yet, deprive the brain of oxygen for a few seconds and consciousness is gone ; remove the thyroid gland and a beautiful and intelligent child becomes a hideous imbecile ; subject the egg-cell of some species to an appropriate salt solution and an individual will develop without a father. The absence of certain chemical bodies prepared inside the body by the thyroid gland, the lack of oxygen to carry on certain necessary combustions in the brain cells—both simple chemical changes—have obliterated the essential characters of the highest form of life. Wherever we look we find life in only one form, composed of compounds of hydrogen, oxygen, nitrogen, and carbon, living between certain narrow limits of temperature, giving out exactly as much energy as it takes in, dependent on the supply of food-stuffs from outside, exhibiting certain electrical properties, poisoned and destroyed by the most absurdly small doses of certain chemical substances."

It is natural for anybody of a sanguine temperament who is impressed by the dependence of life on physical

¹ *British Medical Journal*, May 21st, 1921

circumstances and by the large amount of circumstantial evidence that has been accumulated to think that he is only a short way from the solution of every problem. He thinks he is delivering a final assault on the very citadel of Life itself; then, when the heat of the combat is over and he can look round at what he has accomplished, he finds that it is only an insignificant and almost undefended out-work that he has taken, and the citadel is as far off as ever. Now it is the turn of the pessimist, who says it is no use fighting any more, for the fortress is defended by a great magician against whose spells no material weapon can prevail. Let us, therefore, he urges, see if we cannot use a little magic ourselves and defeat the adversary with his own weapon. On the one side the Mechanist runs round in circles uttering hoarse cries of victory, on the other the Vitalist crouches over his cauldron muttering incantations; and nothing comes of it all. What is a reasonable man to do?

The first thing the reasonable man must do is to be content with a very little knowledge and a very great deal of ignorance. The second thing he must do is to make the utmost possible use of the knowledge he has and not waste his energy crying for the moon. The third thing he must do is to try and see clearly where his knowledge ends and his ignorance begins. Perhaps these are counsels of perfection, but they are better than nothing.

It so happens that no accurate knowledge of the external world has been attained as yet without the use of processes of measurement, which, as we have seen, involve by the use of mechanical devices the actual observation of geometrical variables only. As it further happens that by far the greatest body of known laws are those of physics and chemistry, and that of these the simplest and most general are the laws of motion, it follows that something very like "metrical macroscopic mechanism" is true of the general body of exact know-

ledge at the present day. If anybody wishes to break away from this tradition, and investigate living things or any other portion of the world by other means of investigation there is no theoretical objection to his doing so. There is only the purely practical objection that he has an extremely difficult task before him, in which nobody appears to have succeeded as yet. The plea that the ordinary "mechanical" method of investigation never tells us all we want to know about living things (or indeed about anything else) and that, therefore, we must abandon it, is obviously absurd. It is as though a man refused to eat any food because he could never eat all the food in the world. The important point is that the "mechanical" method gives us some knowledge and in fact gives us very nearly all we have. It is no use quarrelling with it because it is not perfect.

Let us consider an instance of a very successful generalization about living things, Mendel's law of inheritance. It is not obvious that it has any logical connection with the laws of motion, but it is clearly a discovery of a type that can fairly legitimately be called "mechanical" because it is of very much the same general form as mechanical laws. In the first place it involves processes of measurement of a sort, in the second place it is assumed that the organism can for purposes of study be treated as a mere aggregate of isolable characters. This is the kind of assumption against which the criticism of "Vitalists" is likely to be directed (processes of measurement they can put up with to some extent). The criticism is, I think, entirely misdirected as against the legitimate use of such an assumption. It is obviously the simplest assumption to make from the point of view of method and if it is false in any particular case, as it sometimes is, its falsity will be apparent, and allowance can be made for that particular case. For instance the heritable characters an animal possesses are not always found to be quite indifferent to one another, but some have complex

inter-relations which are gradually being unravelled by investigators. Because the original assumption oversimplified the case there is no need to give up in despair, it is merely necessary to introduce suitable modifications. It is obviously always necessary to start by assuming that the case investigated is absolutely as simple as possible. If it is not so, no serious disaster follows. But if we assume that a case is more complex than it is, not only do we make things harder but we are likely to falsify the whole statement of the problem and to get irretrievably muddled up. Criticism of Mendelian methods of investigation on the ground that they do not give us a true insight into the nature of living things and oversimplify the problems, are obviously beside the mark.

This question as to whether living things can legitimately be treated as *atomic* systems or as *organic* is one of importance. An *atomic* system is one in which the laws of the parts composing the aggregate are not altered by the fact of their being parts of the aggregate so that the laws of the aggregate as a whole are some simple function of the laws of the independent parts. An *organic* system on the other hand is such that the parts act differently when combined and in isolation, so that the law of the aggregate may not be a function of the laws of the separate parts at all, or, if it is, is a very complex function. In any investigation it is a great simplification and convenience to be able to treat the system dealt with as atomic and not as organic because the investigation of an organic system is intolerably difficult. But in the case of living beings it is not safe to assume that they are atomic in any respect until we have got positive evidence that they are. On the other hand, as just mentioned, the errors that spring from the false assumption of atomicity are really not so serious as those likely to spring from the opposite assumption. It must be remembered too, that it is not necessarily only living things as such that are organic—if they are

—but there is no definite evidence as yet to prove that complex chemical compounds may not be organic in this sense.¹

Those who have contended, with Dr J. S. Haldane, that the living organism must be considered as a whole or as a unity, have been contending for some such point as this, I think. But to say that the organism must be treated as a whole is a very bad way of putting it, because in those respects in which it has to be treated in this way it is very difficult to discover anything about it. Haldane's own researches are a very valuable example of how much knowledge we can obtain about the whole organism by considering its parts in isolation.

Those biologists (*e.g.* Prof. J. A. Thomson) who have argued for the autonomy of biological sciences, have been putting forward another important point, that in other connections has been mentioned already. That is, that every region of nature must be first examined independently in order to find the special laws of that region before it is any help applying general laws from other regions. It is only after preliminary work in which ordinary physical laws have no part that we can apply general physical laws to biological or any other studies.

§ 33. There is one respect, in which a valid criticism of "mechanical" methods can be made on the ground that they are inadequate. There is a class of questions about natural objects which can be asked and in a few cases answered, and the very existence of which is not contemplated by mechanical methods. These are questions that ask "What for?" Teleological questions in fact, though the word teleological has been so much abused that it is better to leave it out of account altogether. It is absurd to ask what the North Pole is for, but it is not absurd to ask what the Forth Bridge is for. In fact it is possible to answer that question fairly satisfactorily. The Forth Bridge is for trains to go over. The reason

¹ Cf. Broad, *Proc. Arist. Soc.*, Jan. 1919, p. 112

for the difference is that the Forth Bridge is a machine or instrument and the North Pole is not, as far as we know. It is therefore as absurd to ask what the North Pole is for as it is to ask what is the colour of the smell of cheese. In either case the reply can be only a figure of speech. To return to the Forth Bridge, the statement that the bridge is for trains to go over is rather elliptical. What is meant can be expanded into some such form as this, "Men want to travel between Edinburgh and Dundee as quickly and easily as possible, and the Forth Bridge has been constructed to fulfil this purpose. This it does satisfactorily." That is to say in order to answer the question "What for?" we have to say for what human purpose. If there is no human purpose known in connection with the subject of the question, the question as a rule cannot be answered, because we know extremely little about purposes that are not human, or about any other sort of mental activity that is not human. Nevertheless it appears legitimate to ask questions of this sort in a number of cases where there is obviously no human purpose involved. When I say it is legitimate to ask the question, I mean simply that even though no answer is known there is a reasonable expectation that an answer could be given if we were able to carry out the requisite enquiry. Thus it is legitimate to ask what was the purpose of the statues on Easter Island, though no answer is known; but it is not legitimate to ask what is the purpose of a solar eclipse, though at one time people who treated eclipses as Signs or Portents thought they knew the answer. The particular cases I wish to discuss in which questions involving a purpose, but not a human one, appear to be legitimate is the case of the actions and bodily structures of animals and plants. Thus we call certain parts of an animal's body "organs" which means nothing but instruments. A man's hands and kidneys are instruments in that they subserve his purposes just as machines do, the only difference is that the hands

subserve mostly conscious purposes, the kidneys unconscious purposes. If this is true of a man's hands and kidneys, surely it is true of a frog's hands and kidneys even though he has hardly any conscious life at all. The same arguments that apply to bodily structures apply to actions. Thus we say a man eats to assuage his hunger, and we may presumably say the same of a frog.

Now there are two points that must be made clear. The first is that no amount of description of things in terms of physical laws can tell us what anything is for. There is nothing in any law of Physics to tell us that a clock is for keeping time. The laws of nature tell us that the hands go round at a particular uniform speed and that is all. But the second point is that though a knowledge of natural laws cannot from the nature of the case tell us what anything is for, it is much more useful for the purpose of controlling and predicting events. A child may know what a clock is for but he cannot put it right. But a clock could in some cases be put right by a mechanic who did not know what it was for. Strictly all he needs to know are certain mechanical laws and the fact that the hands are required to move at a certain speed. The sort of consequences that follow from a knowledge of the answer to questions about purposes are different from those that follow from knowing the answers to "mechanical" questions. If I want to meet Jones when he is in London it does not matter whether he is going there to see his aunt or to go to the theatre, what I want to know are the mechanical facts as to when he arrives and at what station. On the other hand if I want to persuade Jones to go to London with me it is important to know what his purposes are with respect to his aunt or the theatre.

The laws of nature hitherto discovered tell us nothing about the purpose of anything but they tell us a lot that we very much want to know. If any man of science set himself to try and answer questions about purposes

as well as the other sort there seems to be no harm in it, but he ought to be clear that he is embarking on an entirely new type of enquiry and that any results he arrives at will not do much to enlarge his knowledge in that direction, that an investigation of natural laws along ordinary lines would. As far as I know Historians are the only people who are occupied with answering both types of question, the "mechanical" and the purposive. The historian wants to know not only *what* Mary, Queen of Scots, did on the evening of the 9th of February 1567 but also *why* she acted so, what were her motives. Some historians, I believe, claim that what they study is a branch of Natural Science, but it seems doubtful. At any rate historians seem to deal almost entirely with particular facts and hardly at all with laws.

It would, of course, be very unwise to try to lay down the law about it, but it may be suggested that the scientific study of living things does not for the most part depend for its progress on our ability to answer questions about purposes. It is possibly sufficient to recognize the fact that in certain cases there are such questions. What makes it difficult to be quite sure about the matter, is not the arguments of the Vitalists, but the actual practice of biologists. Biologists do frequently use expressions which imply a knowledge of purposes and sometimes treat such knowledge as "explaining" things. For instance, the strange and disastrous migrations of the lemmings of Scandinavia, are events that obviously ask for an explanation of this sort. Apparently we must assume that the lemmings are actuated by some queerly perverted motive for racial preservation when they are overcrowded or food is scarce in their ordinary habitat. But it is hard to see what is attained by either asserting or denying such "explanations". They may be mere figures of speech. But in any case it does no harm to acknowledge that there is something we can only call a motive or purpose

at work among the "minds" of the lemmings though it may be quite impossible to say anything definite about it.

That living things have all got minds seems a very wild assertion and a dangerous one too. It is only too obvious that many biologists who call themselves Vitalists fight shy of it. Most people would admit fairly readily that the higher vertebrates we are familiar with, dogs and cats and horses and so on, have minds and might even allow frogs and fishes and lobsters to have minds. But when it comes to sponges and cabbages, it seems absurd. The fact is, all knowledge of minds is based on analogy with our own minds, or more precisely between the systems formed by our minds and bodies and other minds and bodies, and where the bodily analogy is remote or apparently non-existent the mental analogy breaks down too.

There hardly seems to be any sufficient basis of evidence on which to discuss the question whether living things in general have minds, and this question is best left unanswered. But it does appear to be a fact there are animals that have minds and investigation of those animals, of whom man is one, cannot profess to be complete unless account is taken of this fact. If we are going to take account of minds as part of the external world it is open to us to investigate them and elucidate their laws in the same way that we investigate physical laws. The fact that such studies have made little headway as yet does not affect the question. The study of Psychology, whatever may be said against it, does seem to have got far enough to vindicate itself as a possible and rational branch of science. Admittedly we are faced with very great difficulties when we come to consider what is the relation between the objects the psychologist talks about—mental states and processes and the purely physical neural processes the physiologist talks about. It is clear that the connection is most intimate, so much so that language does not distinguish

steadily between the two sorts. The Physiologist uses words that are strictly applicable only to minds, with which he is not concerned, and the Psychologist words that are strictly applicable only to brains, with which he is not concerned. This certainly leads to some confusion, whether it also leads to serious fallacies is not so easy to say, but it looks as though it ought to.

The things of the physical world that common sense and science deal with are parts of everybody's experience or have definite relations to experience. This is not the case with mental things. It is only the events in a man's own mind that come within the range of his experience. What happens in minds other than his own, he can only get at in a very roundabout way, utilizing what he knows about his own mind, and a symbolic correlation between mental and physical events. The symbolism is that of bodily expressions and gestures and more particularly of words. Thus we say that a dog wags his tail when he is pleased. There is no causal or functional relation between a wagging tail, which is a complex of events in the physical world, both for the dog who is wagging it and for the onlooker, and the state of feeling pleased which is the dog's peculiar and private mental property. But there is a symbolic relation which is understood by all dogs and men. To a cat, who expresses pleasure in a different way, it is probable that a dog's action in wagging his tail is rather puzzling. The interesting question about all this is how human beings who cannot wag their tails come to understand canine symbolism. It is presumably because we know what sort of things please us and consequently infer, *mutatis mutandis*, what sort of things please dogs and we observe that on the occasions when we expect dogs to be pleased they are usually wagging their tails. All knowledge of the mental states and processes of others must obviously begin in this kind of way. This is not a matter that it is necessary to pursue any further. The point I want to emphasize is that the relation between

the bodily expression of states of mind and the states of mind is arbitrary and conventional, so that nobody, who had not got a mind and body of the right sort himself, could discover the relation. In order to provide the necessary basis for a system of knowledge of this kind it is not only necessary that the observer have a body and a mind, but that there be a mind correlated with the observed body. If somebody made a machine in the likeness of a dog, so constructed that it wagged its tail when it was stroked, we should be mistaken in thinking that this dog had a mind, if that means a mind, related to its body in the same kind of way that a man's mind is related to his body, whatever that may be. But we should be perfectly right in supposing there was a mind involved in the business somewhere, namely the mind of the man who constructed the imitation dog.

There are clearly cases where we may think we find a body-mind symbolism but without sufficient reason. Some people have thought that eclipses formed part of such a symbolism, that they were signals from some superhuman power and had relevance to human fortunes. This theory is not now believed at all widely. The cessation of this kind of belief might be attributed to the discovery that eclipses are due to the operation of mechanical laws. If this is so, I fear I am unable to follow the argument. It is reasonable to suppose that the works of Shakespeare were written by him in accordance with the operation of mechanical laws, but this is not usually taken to imply that Shakespeare had not got a mind or that his works are meaningless. In the matter of eclipses there is of course a much better argument against the theory that they are parts of a body-mind symbolism. If an eclipse is treated as a signal from some god to men, the god is singularly unsuccessful in getting his message through. Nobody knows what it means, and few care. A symbolism nobody understands is not really a symbolism at all. The real argument against the predictions in Old

Moore's Almanac which profess to be based on the motions of the stars is not that these motions are governed by mechanical laws, but simply that the predictions are not true.

It follows from all this that the evidence for the existence of other minds does not differ widely from that for natural laws. If we fail in establishing a system of symbolism as between alleged minds so that our predictions about the actions of the other person based on this symbolism are falsified, the evidence in favour of the existence of other minds is in this case weakened. If a dog when he wags his tail is just as likely to bite a man's hand as to lick it, it tends to throw doubt on the theory that he has a mind, in so far as the tail-wagging symbolism is part of the evidence. Of course it may be simply that the dog is mad or a born liar. On the other hand we can say that a uniformly successful symbolism, such as language among men, is very good evidence for the existence of other minds and is as good evidence as we can possibly hope for from the nature of the case. But it obviously all depends on our certainty of the existence of our own mind and any detailed knowledge of other minds is based on analogous detailed knowledge of our own mind. That indeed seems to be the great difficulty about psychology at present, that you cannot get to work directly and observe a mind but have to interpret other minds in the light of your own, and when that light is mostly darkness, the prospect is sombre.

There is a hope that the advance of knowledge of the physiology of the brain will enable us to some extent to get round the difficulty, but at present this is not much more than a hope. And there is a further difficulty that mental processes cannot reasonably be confined to the class of conscious processes.

We may be allowed to extend the bounds of scientific enquiry to include the study of minds. Can we also include the study of what are vaguely known as miracles,

supernatural, and spiritualistic phenomena? The reply must be, yes, if they can be studied by the methods of observation and experiment and if no fundamental assumption is required which is contrary to what is generally held to be true about the external world. That is to say, we must be prepared for the possibility of finding what we think are laws of nature break down in special cases, but we must not start off with the assumption that any are false. We must also follow ordinary methodological rules and refuse to accept a complex hypothesis or one invented merely *ad hoc* for a special case, unless there is no alternative. For instance the process known as 'levitation' for the reality of which there seems to be quite respectable evidence is obviously quite amenable to investigation by ordinary scientific methods, and if investigated might prove of considerable interest. All the alleged occurrences of a spiritualistic character are clearly open to investigation. The immense amount of imposture and humbug that surrounds the business makes proper investigation much more difficult than it would otherwise be if people who want to be imposed upon had left the matter alone, but really it only makes rational treatment all the more necessary, because there seems to be at least a residue of facts for which there is no proper explanation in terms of existing knowledge.

There is only one sort of thing that can never be dealt with by the ordinary methods of science, that is what is unique. It is of the essence of the method of investigation that occurrences should be capable of repetition under controllable conditions. Therefore that aspect or part of anything which is unique and not capable of being reproduced is outside the range of investigation. It is obvious that this must be so and equally obvious that very little is lost by this limitation. If something can never happen again the kind of knowledge we require about it is obviously very different from the kind of knowledge we want about things that

may happen again. We want to know what happens when we take arsenic, because we might take it any day, but we do not want to know it in at all the same sort of way, what happens when Julius Caesar dies, because he only dies once. If we are interested in the death of Caesar it is because we are interested in the particular fact itself and in the human emotions and desires connected with it. We know that the class of events "Deaths of Julius Caesar" has only one member and is of no scientific interest. Our scientific curiosity can only be roused if we treat it as a member of a class that has many members—deaths by violence, assassinations of tyrants, last hours of great men, and so on—all these are legitimate subjects for scientific treatment.

In conclusion we may say that scientific method is simply the attempt to acquire knowledge of general laws directly or indirectly by experience, by the use of our five senses. The only limitations that can be assigned to the applicability of this process are those due to the character of experience. Anything that is logically related to experience by discoverable laws and is capable of description in general terms can be dealt with by the scientific method. Where precisely these limits are to be drawn it may be difficult to determine and is in any case quite unnecessary.

CHAPTER VII

Conclusion

§ 34. IT has been pointed out by Bergson that Greek Science was concerned chiefly with *genera*, whereas modern science is concerned with *laws*. For Plato and Aristotle the genus 'horse' was not simply the collection of all horses, not *every* or *any* horse, but it was the *good horse*. It is customary, but misleading, to translate their notion as the *ideal horse*. In modern language an idea means a thought, and an ideal thing is something simply thought of or imagined as opposed to something real or existing. This was not at all what the Greeks meant. It was not even what a modern biologist would call a *type*. The typical horse or the type of horses is simply a sort of average of horses; it is not the same as *any* or *every* horse, but it is a notion derived from them. The type consists of the common characteristics of *any* large collection.

The conception of the *normal* individual comes nearer to the old idea than any other modern scientific notion, and this we must discuss to see where exactly ethical considerations come in and how far they are legitimate. When we speak of *normal vision* or the *normal eye* we are not referring to what is true of the eyes of any individual or of every individual, not even of what is *typical* of the whole class. The *typical* condition may quite possibly be abnormal. The normal eye is one in which all mechanical defects in the apparatus of vision are at a minimum. Since the defects are as likely to be in one direction as in another, it will commonly happen that the typical eye in any group corresponds very closely to the *normal eye*. In the

case of the eye we can define accurately and in a manner suitable for scientific investigation what is meant by normality, and it is clear also that the *normal* eye is the *good* eye. The good eye as Socrates would say, is the eye that is good for seeing, just as the good cobbler or statesman is the man who is good at cobbling or governing. To discern which eye is good we must be able to state what is the end or purpose of the eye. This we can do because the eye is an instrument or tool, it has a precise and limited function such that we can define accurately the conditions under which it fulfils these functions. But observe, it is only in certain cases that we can define the functions of anything at all strictly. Plato, when he spoke of the good man or the good horse, conceived that there was one definite end for men and one for horses and that the end was discoverable and definable. Men and horses were so far like eyes, they were instruments, though, unlike the eye which is an instrument that subserves an external end, their ends might be internal, they might be ends in and for themselves. Plato was at great pains to discover and define the end and function of man both as an end to himself, and as part of the community. He made the attempt because he conceived of the end as being fixed and eternal and because he thought that men in spite of their differences could all fit in as organic parts of a single society.

The point of all this talk is to bring to light certain snares and pitfalls which beset the introduction into scientific discussion of such a conception as that of normality. We can define the normal eye because the eye is a part only and obviously an instrument, and we know exactly what it is for. We cannot define the normal man unless we know exactly what he is for, and we are not half so sure we know this as Plato was. At any rate, in order to define what is meant by a normal man we must have a complete theory of ethics, politics and religion, which is at present lacking. More-

over, so far as our theories go they do not suggest that the end of man is necessarily single or fixed. We are prepared to believe that there may be many different ends and goods for different men; and we do not admit this plurality of ends only in Plato's sense, that individuals with different capacities have different places and different functions within one society. It may be that there are different and even incompatible goods for men, so that they cannot successfully co-operate in one society, although each man is capable within his own sphere of attaining his own perfection. Thus the end of the European may not be the same as that of the Asiatic or the Negro and their ends may be incompatible as long as they all try and form members of one community. But each apart may be able to pursue his own ends in a society of his own race.

This conception of normality, which is fundamentally ethical, must be used with great caution. It is legitimate for scientific purposes only if we can define the end and determine by observation how far any individual deviates in any direction from its attainment. We cannot do this at all successfully in the case of a whole organism such as a man or a horse, but we can in the case of certain parts of organisms when these are considered in relation to the whole.

Considering the function of the eye and what are the conditions of normality helps us to understand the whole organism of which the eye is a part, but it does not help us to find out anything about the eyes themselves. We can only discover how eyes work by observing actual eyes, and the conception of normality is of no help. If we did happen to know what the end of man was, and what men were normal with respect to it, it would tell us nothing about actual men though it might tell something about the state or about God. If the conception of normality is to be useful and not entirely vicious in biological investigation these limitations must be kept in view.

In actual practice the conception has been used in a very muddled way. It is usually mixed up with the notion of the *type* already mentioned, which lends to it a certain degree of plausibility. The trouble comes from not considering at the outset of an investigation what it is that is wanted. In dealing with living things you may investigate (1) the ends for which they live and how far they fulfil them, or (2) the way their bodies and minds are constructed and work. It does not seem possible to prosecute both investigations at once. For investigation (1) the idea of normality may be useful, for (2) the idea is of restricted use when it is applied to the relation of the parts to the whole, but the idea of the normality of the whole organism will not help us to find out anything about that organism. On the other hand ideas such as that of the typical organism are of use for investigation (2) but not for investigation (1). Further it must be remembered that investigations of type (1) have not in the past been so successful as those of type (2).

The notion of a class, then, which is likely to be valuable for scientific reasoning along traditional lines, will exclude such extraneous ideas as we have been discussing. That is to say, a class is simply a collection of "things" which have as a matter of fact certain relations and characters common and peculiar to themselves. The laws which it is the business of investigation to establish are the relations or characters which are found, as a matter of fact, to be true of any or every member of the collection. Emphasis is laid on the necessity of treating the characters and relations of classes as matters of fact to be discovered by observation and not asserted as a matter of definition. We can, if we like, make classes by definition and find laws among them by deduction, but the result will not be scientific knowledge; it may possibly be pure mathematics but it is more likely to be pure nonsense.

We can speak of classes and relations, of universals in this sense, without prejudice to the questions at issue between Realists and Nominalists. The question whether there is an essence which exists apart from individual horses, is one that loses most of its interest and importance when one gives up the old 'ethical' view of the nature of kinds. In fact, it becomes little more than a matter of words. On the other hand, it makes Conceptualism rather a dangerous doctrine. We may reasonably attribute the individuals and the collection of them to one real world, or we may say the individuals are essential and the collection merely a name or sign. But if the individual is an essence in the external world, and the class or collection an essence in the mental world, a new and horrible form of the bifurcation of nature seems to be suggested.

This question of the meaning of normality has been discussed as illustrating the troubles encountered when anybody incautiously oversteps the traditional limitations of scientific method without realizing what he is doing. A great deal of theorizing in biological science is vitiated by the introduction of extraneous ideas and a great many pseudo-problems have been created. Fools are constantly rushing in where Darwin took care not to tread.

§ 35. People have noted with admiration how the progress of scientific enquiry is like the growth of a coral reef; each generation of little toilers building a sure foundation on which their successors may build yet further. The simile is apt in many ways, and in one way in particular that is worth considering. When we see how industrious and how prolific are the coral insects, our chief astonishment should be, not how vast are the structures they have built, but how few and scattered. Why is not every coast lined with coral? Why is the abyss of ocean not bridged with it? The answer is that the coral only lives under certain limita-

tions ; it can only thrive at certain depths, in water of certain temperatures and salinities ; outside these limits it languishes and dies. Science is like the coral in this. Scientific investigators can only work in certain spots of the ocean of Being, where they are at home, and all outside is unknown to them. Of what may be outside the region of the known it is forbidden to me to speak ; others more bold may conjecture that it is finite or infinite according to their fancy. If any of the larvæ, to return to our simile, drift away from their homes, most will perish and leave no trace, but some may find a rock on which they can build and found a new colony. Physical science has occupied the most fertile regions of enquiry, but outside these regions little colonies of knowledge are to be found, some flourishing and some languishing. We do not know how far afield these colonies may reach in future, nor do we know if they are all founded on the same reef and will ultimately join to make one structure. In order to keep away from what is only guesswork we must pay attention chiefly to the main structure of knowledge, physical science. When we find that this knowledge has developed under certain limitations it is necessary to decide how much of the limitations is a matter of necessity, how much a matter of convenience, and how much merely accidental. Is growth possible under other conditions or only difficult? If it is possible how much will change in the environment, change the form of the structure? All these are difficult questions to answer.

Notwithstanding the difficulties of the subject, certain philosophers have made a great show with discussions of the supposed limitation of scientific enquiry. They have argued that science cannot do this, and science cannot do that, and *therefore* some system of philosophy is true. Sometimes they seem to talk sense but more often nonsense ; at any rate they have all a metaphysical axe to grind ; they want to prove something about the

ultimate nature of things.¹ There is no objection to metaphysics as such, quite the contrary; in its own place it is admirable. All metaphysical systems have originated in some view as to the nature of the whole universe, that it is One or Many, that it is ultimately Pure Thought or Pure something else. They have also involved some view of the nature of the Good. Any validity a metaphysical system has it derives from some such dogma. Beliefs of that sort are not obtained through experience by means of the five senses. Neither are they laws of logic, *i.e.* formal propositions as to the nature of propositions; for they are not formal at all but material. They are not hypotheses put forward to be rejected or confirmed by experience, for experience has no part or lot in them, and can neither reject nor confirm. They have no basis in common with the propositions of science. That is not to say they have no basis at all, but that they are based on 'intuition' as it may be called for lack of a better name. Nobody, however hard he tries, can avoid having some metaphysical beliefs. If he does not work out his metaphysics systematically he does not avoid any difficulties, he merely represses them. Unconscious metaphysics is pretty sure to be bad metaphysics.

Therefore, I am not quarrelling with metaphysics, only with its misuse. I consider it a misuse when it is asserted that the results of scientific enquiry or the failure of scientific enquiry to produce any results have any metaphysical bearing, or when the supposed limitations or fundamental assumptions necessary to scientific enquiry are asserted to have such a bearing. By a metaphysical bearing, I mean, either that some particular metaphysical theory is supposed to gain

¹ I need hardly say I am referring to M. Bergson and his followers, and to certain thinkers of a Hegelian tendency, such as Lord Haldane. As the writings of these authors are incomprehensible to me I am naturally not able to criticize them. I can do no more than apologize for the fact that all their eloquence and learning leave in my mind only bewilderment and dumb hostility.

credibility from science, or on the other hand, that metaphysical theories somehow imply scientific ones, so that the metaphysician can say "I told you so" to the scientific man. In short, I deny that there is any logical relation between scientific and metaphysical propositions. The two sets are, or should be, entirely neutral to one another.

That there is ever any confusion is due largely to the paucity of language that uses one word for so many different things, but there are other sources of confusion too in the human weakness that makes us think a knowledge of the part always tells us about the whole, and that things we would like to exist do exist. Whatever the reason for the confusion may be, it is time it stopped, and that scientists who burn their fingers with metaphysics and metaphysicians who burn theirs with science cease to blame one another and realize it is all their own fault. Having delivered this sermon, I can only hope and pray that I have not been guilty of any like confusion.

Some of the less cautious scientific men used to think, and some perhaps think still, that their investigations were going to tell them all about everything. They confessed, of course, that they were still not quite clear over a few minor points, but, anyway, the main outlines were sketched out all right. They supposed that they were regarding the whole universe sub specie eternitatis whereas they were looking at a few little bits of one corner of it under a quite flagrantly temporal aspect. That this crude notion is being given up is not due to the taunts of the philosophers, though these were well grounded enough, but to force of circumstances, that the high and mighty attitude led to contradictions. That the man of science has to recognize his limited point of view is nothing for anybody to be pleased about. The recognition of limitations is always a nuisance because it complicates matters. The fortunate thing is that the limitations as

far as they are understood make so little difference to most of the results, that we do almost as well as if we were really regarding the world *sub specie eternitatis*

Now the decision as to what are really the limitations and foundations of scientific enquiry is a technical matter that must be settled by scientific methods, not by any appeal to metaphysics. Whatever justification there may be for asserting the principle of uniformity as a dogma of metaphysics there is none from the scientific point of view. Therefore, it must be left out of consideration, even if it is true.

There are certain matters where scientific and philosophical investigations touch. For instance the difficulties due to what Whitehead calls the Bifurcation of Nature and attempts to get over these difficulties by such a theory as Phenomenalism are matters of interest both to the philosopher and to the scientist who is concerned with the foundations of his theory. But these questions do not or should not involve any appeal to metaphysics in the sense in which I have used the word.

These questions need not detain us as they have been sufficiently dealt with by abler thinkers.

§ 36 At the outset of this essay I urged the necessity of avoiding metaphysical presuppositions as far as possible, and in this final chapter have returned to the subject. Therefore, it is only fair to be honest about my own metaphysical presuppositions, so that the intelligent critic can make allowance for them. What follows is merely a personal apology, and is therefore of no scientific interest.

In our exploration of the external world, we have to reject or neglect a very large part of what is given in experience in order to be able to make use of the remainder. What we do make use of has to be treated by very elaborate processes before the raw material of experience is converted into the finished product of scientific theory. When the process is all over, it is

very difficult to see the connection between what we started with and what we finish with. The world of immediate experience is an affair of blurred and fluctuating outlines, of meaningless variegation, it is full of loose ends, of vague relations to something outside its immediate content. Out of this we construct something neat and tidy, something compact and rounded off. Yet we are compelled to believe that the last is somehow a true reflection of the first. It may be a distorted reflection, but the distortion goes by rule, so that the image is veridical even if it is not veritable.

The process of selection and abstraction, of trial and failure by which our knowledge has been slowly and painfully acquired provides genuine but partial and fragmentary information. Some thinkers are so much preoccupied by the genuineness of the information that they think it must be complete. Others are so pre-occupied with its incompleteness that they cannot believe it to be genuine. But there seems to be no reason to agree with either party. It seems clear to me that the order in nature of which science reports is really there, and is not a mere figment. But it seems to me equally obvious that the orderliness is not all pervasive. There are streaks of order to be found among the chaos and the nature of scientific method is to seek these out and to stick to them when found and to reject or neglect the chaos. Men take great trouble to obtain what they want even if it is scarce and take no notice of what they do not want even if it is abundant. A visitor to this earth from another planet seeing copper and tin handled in large quantities, would think these metals were abundant constituents of the earth's crust. He would never guess that they are present in mere traces relative to the whole bulk of attainable material. Neither would he suppose that titanium, which he would never see except perhaps in tiny scraps in some museum, formed an appreciable portion of the earth's crust. However abundant chaos or complexity may be it is of no use

and nobody will trouble to notice its presence. However scarce order or simplicity may be it will be sought for until it is found. As Poincaré points out, we analyse our data just so far as to obtain simplicity and no further. “Il faut bien s’arrêter quel que part, et pour que la science soit possible, il faut s’arrêter quand on a trouvé la simplicité.”¹

Now it is obvious that we have succeeded in finding some order in nature, but this fact in itself does not prove anything further. It suggests that having found some order, it is worth looking for more, but it does not imply that nature is orderly through and through, though, of course, it might be so. Nevertheless, the extreme difficulty and labour of finding laws of nature even when you know where and how to look, much more when it is a question of discovering a new one, suggest that there is not so much simplicity and order about as people think. If nature were really orderly through and through, it is probable that Bacon’s notion would be correct, that given the correct procedure, any fool could discover laws; but this is notoriously not the case. It is only as the result of the century-long labours of the wisest of men that anything much has been discovered, and that as the result countless mistakes and false starts. Even when we know what the laws of nature are, it is not such an easy business to observe their correctness in any instance, but it needs special training and special means to carry out the observations.

The fact that the regions of nature actually covered by known laws are few and fragmentary is concealed by the natural tendency to crowd our experience into those particular regions and to leave the others to themselves. We seek out those parts that are known and familiar and avoid those that are unknown and unfamiliar. This is simply what is called “Applied Science”.

That our knowledge only illuminates a small corner of the Universe, that it is incomplete, approximate,

¹ *La Science et L’Hypothèse*, p. 176

tentative and merely probable, need not disconcert us. It is genuine nevertheless. Physical science stands as one of the great achievements of the human spirit.

“Every truth is a shadow, except the last; but every truth is a substance in its own place, though it be but a shadow in another place; and the shadow is a true shadow, as the substance is a true substance”.

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